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Astronomical





PUBLICATIONS
OF THE
ASTRONOMICAL SOCIETY
OF THE PACIFIC.

VOLUME VI.
1894.

SAN FRANCISCO
PRINTED FOR THE SOCIETY.
1894.

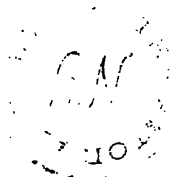
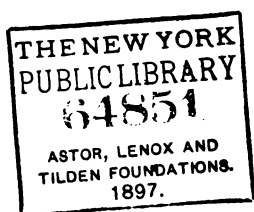


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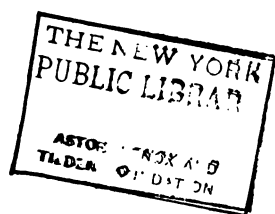
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Rev. GEORGE STOKES	{ St. John's Vicarage, Isleworth, Middlesex, England.
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Prof. W. H. VON STREERUWITZ	P. O. Box 465, Austin, Texas.
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Miss M. J. TURNER	11 Faxon Ave., Quincy, Mass.
Prof. L. W. UNDERWOOD	UNDERWOOD Observatory, Appleton, Wisconsin.
Miss SARA CARR UPTON	2109 Pennsylvania Ave., Washington, D. C.
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Mr. FELIPE VALLE	National Observatory, Tacubaya, Mexico.
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Rev. JACOB VOORSANGER	2316 California St., S. F., Cal.
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Mrs. W. SEWARD WEBB	680 5th Avenue, New York, N. Y.
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Mr. J. HOWARD WILSON	753 Carroll St., Brooklyn, N. Y.
Mr. WILLIAM C. WINLOCK	Smithsonian Institution, Washington, D. C.

Colonel B. WITKOVSKY*	{ St. Petersburg, Petersburg Side, Great Prospect h. 8, l. 5, Russia.
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Mr. CHARLES G. YALE	{ Statistician U. S. Mint, S. F., Cal.
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Prof. C. V. ZENGER	Smichow, Prague, Bohemia.
Mr. F. R. ZIEL	410 California St., S. F., Cal.

LIST OF CORRESPONDING OBSERVATORIES AND INSTITUTIONS.

[Arranged Alphabetically by Cities.]

Dudley Observatory, Albany, New York.
 Observatory, Algiers, Africa.
 Allegheny Observatory, Allegheny, Pennsylvania.
 Lawrence Observatory, Amherst, Massachusetts.
 Detroit Observatory, Ann Arbor, Michigan.
 Observatory, Armagh, Ireland.
 Library, University of California, Berkeley, California.
 Redaction Berliner Jahrbuch, Berlin, Germany.
 Royal Observatory, Berlin, Germany.
 Gesellschaft Urania, Berlin, Germany.
 Observatory, Berne, Switzerland.
 University Observatory, Bonn, Germany.
 Observatory, Bordeaux, France.
 Royal Observatory, Uccle, Brussels, Belgium.
 University Observatory, Cambridge, England.
 Harvard College Observatory, Cambridge, Massachusetts.
Royal Observatory, Cape Town, Africa.
 Astronomical Society, Chicago, Illinois.
 University Observatory, Christiania, Norway.
 University Observatory, Cincinnati (Mt. Lookout), Ohio.
 Litchfield Observatory, Clinton, New York.
 National Observatory, Cordoba, Argentine Republic.
 Chamberlin Observatory, University Park, Denver, Colorado.
 Astronomical Society of Michigan, Detroit, Michigan.
 University Observatory, Dorpat, Russia.
 Trinity College Observatory, Dublin, Ireland.
 Royal Observatory, Edinburgh, Scotland.
 Lick College, Fredericksburg, Lebanon Co., Pennsylvania.
 Observatory, Geneva, Switzerland.
 College Observatory, Georgetown, District of Columbia.
 Morrison Observatory, Glasgow, Missouri.
 University Observatory, Glasgow, Scotland.
 Ducal Observatory, Gotha, Germany.
 Royal Observatory, Greenwich, England.
 Observatory, Hamburg, Germany.
 Shattuck Observatory, Hanover, New Hampshire.
 University Observatory, Helsingfors, Russia.
 Ducal Observatory, Karlsruhe, Germany.
 University Astronomical Observatory, Kasan, Russia.
 University Observatory, Kiel, Germany.
 University Observatory, Königsberg, Germany.
 Royal Observatory, Copenhagen, Denmark.
 Observatory, La Plata, Argentine Republic.

University Observatory, Leyden, Holland.
Astronomische Gesellschaft, Leipzig, Germany.
University Observatory, Leipzig, Germany.
Royal Observatory, Lisbon, Portugal.
Astronomical Society, Liverpool, England.
British Astronomical Association, London, England.
British Museum, London, England.
Royal Astronomical Society, London, England.
The Nautical Almanac, London, England.
University Observatory, Lund, Sweden.
Observatory, Lyons, France.
Washburn Observatory, Madison, Wisconsin.
Observatory, Madras, India.
Royal Observatory, Madrid, Spain.
Observatory, Marseilles, France.
Observatory, Melbourne, Victoria.
Stanford University, Menlo Park, California.
Royal Observatory, Milan, Italy.
University Observatory, Moscow, Russia.
Lick Observatory, Mt. Hamilton, California.
Royal Observatory, Munich, Germany.
Royal Observatory, Naples, Italy.
Observatory, Neuchâtel, Switzerland.
Yale University Observatory, New Haven, Connecticut.
Observatory, Nice, France.
Observatory of Carleton College, Northfield, Minnesota.
Chabot Observatory, Oakland, California.
Radcliffe Observatory, Oxford, England.
University Observatory, Oxford, England.
Royal Observatory, Palermo, Italy.
Bureau des Longitudes, Paris, France.
Astronomical Society of France, Paris, France.
National Observatory, Paris, France.
Astrophysikalisches Observatorium, Potsdam, Germany.
University Observatory, Prague, Austro-Hungary.
Halstead Observatory, Princeton, New Jersey.
Imperial Observatory, Pulkowa, Russia.
Observatory, Rio de Janeiro, Brazil.
Observatory of the Roman College, Rome, Italy.
Italian Spectroscopic Society, Rome, Italy.
California Academy of Sciences, San Francisco, California.
Mercantile Library, San Francisco, California.
Mechanics' Institute Library, San Francisco, California.
Technical Society of the Pacific Coast, 819 Market Street, San Francisco, California.
Observatory, Santiago, Chile.
University Observatory, Stockholm, Sweden.
University Observatory, Strassburg, Germany.
Observatory of Sydney, New South Wales.

National Observatory, Tacubaya, Mexico.
University Observatory, Tokio, Japan.
Observatory, Toulouse, France.
University Observatory, Upsala, Sweden.
McCormick Observatory, University of Virginia, Virginia.
Imperial Observatory, Vienna, Austria.
VON KUFFNER'sches Observatory, Ottakring, Vienna, Austria.
The American Ephemeris, Washington, District of Columbia.
Library of Congress, Washington, District of Columbia.
National Academy of Sciences, Washington, District of Columbia.
Naval Observatory, Washington, District of Columbia.
Smithsonian Institution, Washington, District of Columbia.
U. S. Coast and Geodetic Survey, Washington, District of Columbia.
Library U. S. Military Academy, West Point, New York.
Field Observatory, Williamstown, Massachusetts.
Observatory, Zurich, Switzerland.

BY-LAWS
OF THE
ASTRONOMICAL SOCIETY OF THE PACIFIC.*

ARTICLE I.

This Society shall be styled the ASTRONOMICAL SOCIETY OF THE PACIFIC. Its object shall be to advance the Science of Astronomy, and to diffuse information concerning it.

ARTICLE II.

This Society shall consist of Active and Life members, to be elected by the Board of Directors.

1. Active members shall consist of persons who shall have been elected to membership and shall have paid their dues as hereinafter provided.

2. Life members shall consist of persons who shall have been elected to life membership and shall have paid \$50 (fifty dollars) to the Treasurer of the Society.

3. A certain number of Observatories, Academies of Science, Astronomical Societies, Institutions of Learning, etc., not to exceed one hundred, shall be designated by the Board of Directors as Corresponding Institutions and they shall receive the publications of this Society in exchange or otherwise.

ARTICLE III.

At each annual election there shall be elected a Board of eleven Directors, and a Committee on Publication, consisting of three members. The officers of this Society shall be a President, three Vice-Presidents, two Secretaries and a Treasurer. The Directors shall organize immediately after their election, and elect from their number the officers of the Society. They may also appoint a Librarian, and such other assistants as may be required. The Directors shall fill by appointment any vacancies which may occur after the annual election.

The Library of the Society shall be kept in San Francisco, and shall be open to the use of all the members.

ARTICLE IV.

The President, or, in his absence, one of the three Vice-Presidents, or, in the absence of both the President and the Vice-Presidents, any member whom the Society may appoint, shall preside at the meetings of

* For the convenience of new members, the By-Laws now in force are here printed.

the Society. It shall be the duty of the President to preserve order, to regulate the proceedings of the meetings, and to have a general supervision of the affairs of the Society. The President is *ex-officio* a member of all Committees of the Board of Directors.

ARTICLE V.

The Secretaries shall keep, and have the custody of, the records ; they shall have the custody of all other property of the Society, excepting the money thereof ; they shall give timely notice of the time and place of meetings ; they shall keep in books a neat and accurate record of all orders and proceedings of the Society, and properly index them ; they shall conduct the correspondence of the Society ; they shall preserve and index the originals of all communications addressed to the Society ; and keep a copy of all their letters, properly indexed ; and they shall prepare for publication an accurate summary of the transactions of the Society at each of its meetings.

ARTICLE VI.

The Treasurer shall receive and deposit in such bank as may be designated by the Directors, to the credit of the Society, all donations and bequests of money and all other sums belonging to the Society. He shall keep an account of all money received and paid by him, and at the annual meetings shall render a particular statement of the same to the Society. Money shall be paid by him only on the written order of the Finance Committee of the Board of Directors. He shall give such bonds as may be required by the Board of Directors.

ARTICLE VII.

Candidates for active or life membership may be proposed by any member of the Society to either of the Secretaries, in writing. A list of such candidates shall be certified to the Board of Directors by the Secretaries at each of their meetings, in writing. A majority (not less than three) of the Directors present at any such meeting shall be required for election.

ARTICLE VIII.

Each active member shall pay an annual subscription of five dollars, due on the first of January of each year, in advance. Each active member shall, on his election, pay into the Treasury of this Society the sum of five dollars, which shall be in lieu of the annual subscription to the first of January following his election, and in lieu of an initiation fee. No one shall be deemed an active member, or receive a diploma, until he has signed the register of members, or accepted his election to membership in writing, and paid his dues for the current year. Any member may be released from annual dues by the payment of fifty dollars at one time, and placed on the roll of life members by the vote of the Board of Directors. Any failure on the part of a member to pay his dues within six months after the time the same shall have become payable, shall be considered equivalent to a resignation.

ARTICLE IX.

The annual meeting of this Society shall be held on the last Saturday in March, at eight o'clock P. M., at the rooms of the Society in San Francisco; and meetings shall be held for the ordinary transactions and purposes of the Society, as follows:

Meetings shall be held in the Library of the Lick Observatory, Mount Hamilton, at a suitable hour on the second Saturday of June and the first Saturday of September; and meetings shall be held in the rooms of the Society, in San Francisco, at eight o'clock P. M., on the last Saturdays of January, March and November.

A special meeting may be called by the President, or, in his absence or disability, by one of the Vice-Presidents, or, in the absence or disability of both the President and the Vice-Presidents, by the Secretary, on the written requisition of ten active or life members; and the object of such meeting shall be stated in the notice by which it is called.

The annual election shall be held on the day of the annual meeting, between the hours of 8:15 and 9 P. M.

No member shall be permitted to vote at any meeting of the Society who has not paid all his dues for past and current years. There shall be no voting by proxy.

ARTICLE X.

Fifteen active or life members shall be a quorum for the transaction of business.

ARTICLE XI.

No papers or manuscripts shall be published by the Society without the consent of the Directors. Any motion to print an address, or other paper read before the Society, or any other matter belonging to the Society, shall be referred to the Committee on Publication, who shall report to the Directors. The Committee on Publication may make suggestions to the Directors, from time to time, with reference to the publication of such papers as in their judgment should be published by the Society; and this Committee shall have the care, direction and supervision of the publication of all papers which the Directors may authorize to have published.

Members of the Society shall receive all the publications of the Society free of charge.

ARTICLE XII.

This Society may, by a vote of a majority of all its active and life members, become a branch of an American Astronomical Society, should one be formed.

ARTICLE XIII.

It shall be the duty of the Directors, in case any circumstances shall arise likely to endanger the harmony, welfare or good order of the Society, to call a special meeting of the Society; and if, at such meeting, after an examination of the charges, and hearing the accused, who shall have personal notice of such proceedings, it shall be proposed that the

offending member or members shall be expelled, a vote by ballot shall be taken, and if two-thirds of the members present vote in favor thereof, the offending member or members shall be expelled.

ARTICLE XIV.

The Directors shall meet half an hour before the stated time of each bi-monthly meeting, and at such other times as they may appoint. The President, or, in his absence, any one of the Vice-Presidents, may call special meetings of the Board of Directors at any time. Notice of the time and place of such meeting shall be given by the Secretaries, by depositing in the post-office at San Francisco a notice of the time and place, addressed to each Director personally, at his last known place of residence, with the postage thereon prepaid, six days before the time of meeting.

ARTICLE XV.

The By-Laws may be amended at any time by a consenting vote of nine members of the Board of Directors at any duly called meeting thereof.

ARTICLE XVI.

In order to increase the usefulness of the Society, any groups of its members residing in the same neighborhood (except in the City and County of San Francisco, State of California) are authorized to form local organizations which shall be known as "The ——— Section of the Astronomical Society of the Pacific."

No Section shall be formed except by the consent of the Board of Directors of the parent Society.

The proceedings of such Sections may be printed in the Publications of the Astronomical Society of the Pacific, either in full or in abstract, and the parent Society shall not be in any way responsible for publications made elsewhere.

No person not a member of this Society in good standing shall be eligible to membership in a Section, nor shall membership in a Section interfere in any way with the status of the person as a member of this Society.

The special expenses of each Section shall be borne by the group of members composing it, and this Society shall not be liable for any debts incurred by any Section.

P U B L I C A T I O N S
O F T H E
Astronomical Society of the Pacific.

VOL. VI. SAN FRANCISCO, CALIFORNIA, JANUARY 27, 1894. No. 34.

THE OCCULTATION OF *SATURN*, MAY 25, 1893.

BY JOHN TEBBUTT.

The steadiness and definition of the Moon and *Saturn*, both at the beginning and at the end of this interesting phenomenon, were remarkably satisfactory. Long before the calculated time of the disappearance the planet could be well seen in the strong twilight, with the 8-inch refracting telescope of this observatory; and a few minutes before the contacts occurred, *Titan* became visible as a speck of silvery light; it was then a few seconds distant from the planet's north limb. A magnifying power of 74 diameters was employed in all the observations, but the twilight was too strong to admit of the Moon's dark limb being seen. Guided by an approximate calculation furnished by Mr. R. T. A. INNES, F. R. A. S., of Sydney, I succeeded in obtaining the following observations, in local mean time, of the principal phases of the phenomenon:

	h.	m.	s.
First contact of the western end of the ring . .	5	16	41.8
First contact of the ball	5	16	50.5
Disappearance of <i>Titan</i>	5	17	3.3
Total disappearance of the ball	5	17	33.8
Total disappearance of the ring	5	17	47.0
Re-appearance of the western end of the ring .	6	36	20.6
The ball began to reappear	6	36	41.1
Total re-appearance of the ball	6	37	20.5
Total re-appearance of the ring	6	37	43.4

The Moon's dark limb, as it passed gradually over the ball of the planet, was sharply defined. The only remarkable phenomenon observed in connection with this occultation was a streak of

copper-colored light which lingered, for a large portion of a second, on the Moon's dark limb at the point of disappearance. After the last trace of the eastern end of the ring had disappeared, the limb was rendered visible for several seconds of arc each way from the point of disappearance. This phenomenon would seem to point to the existence of a low-lying lunar atmosphere, or of a faint luminous atmosphere round the planet itself.

THE OBSERVATORY,
WINDSOR, N. S. W., June 5, 1893.

CONSIDERATIONS ON THE METHODS OF REPRESENTING THE MILKY WAY, SUGGESTED BY A RECENT WORK.*

BY EDWARD S. HOLDEN.

Mr. EASTON's introduction tells us that his drawings of the Milky Way were made in the years 1882-87, and that in their definitive form they refer to the latter year. The work is an entirely independent one. It was not until 1892 that Mr. EASTON compared his designs with the maps of HEIS and BOEDDICKER. This comparison shows differences between the drawings of the three observers; but these differences are such as are usually found between the drawings of the same celestial object by different artists, and in Mr. EASTON's opinion it is quite practicable to obtain a satisfactory agreement in such visual drawings if sufficient pains are taken. Where three observers agree we may assume that all three are right. Where they differ further work is necessary. There appears to be a sufficient discrepancy between certain parts of the drawings of Messrs. EASTON and BOEDDICKER to require some revision of the sort. The nature of the difficulties to be surmounted is familiar to those who have made such drawings, or it can be made plain to anyone who will examine a series of drawings of planets, the nebula of *Orion*, or of regions on the Moon, by different observers.

After the drawings were finished, Mr. EASTON had great diffi-

*C. EASTON: *La Voie Lactée dans l'hémisphère boreal*. (Containing five plates lithographed by the author, a detailed description, a catalogue of stars, etc., an historical notice together with a preface by Professor H. G. v. d. S. BAKHUYZEN, Director of the Observatory of Leyden.) Paris: Gauthier-Villars, 1893. Atlas folio.

culty in finding a suitable method of reproducing them. Methods of reproduction by photography were tried without success, and it was finally decided to lithograph the maps, the author himself making the drawings on the stone. Everyone who has had to do with the problem of producing a large edition of plates, which must be absolutely alike, from a single original (either drawing or photograph), will understand the difficulty. Even the lithographic impressions were not entirely similar, and Mr. EASTON touched up, by hand, the different plates as they came from the press. The atlas is, then, as nearly representative of the original drawings as it can well be made.

No doubt the process employed by Mr. EASTON (and first practiced, I believe, by Dr. LÖHSE in his drawings of Sun spots, in the Potsdam observations) is as nearly satisfactory for reproducing *drawings* as any other. If drawings are reproduced by photography, the very first copy on a sensitive plate changes all the contrasts of the original design. It is usual to send this first negative to the person who is to make the process-block for printing, and who must make another copy on a "stripping-plate," or on something equivalent. These stripping-plates are usually very slow and the contrasts are again much changed by the transfer. Finally, the block is made, and in the course of printing the impressions new changes of contrast come in, not to speak of great losses of definition. If the original is a negative, and not a drawing, difficulties of precisely the same sort are present. Definition is always lost and the contrasts are always changed, more or less. Our experience at the LICK Observatory has been considerable, and we have found reproductions by heliogravure (on copper) to be the most satisfactory. They are hardly more than twice as expensive as the best "processes," and they are very much superior.

Mr. EASTON's remarks on the difficulties of representing the Milky Way by eye-drawings are of importance in themselves and they apply directly to all drawings of faint objects, as comets or nebulæ. Strictly speaking, no two observers see the same Milky Way. Nor do two different photographic telescopes, plates, foci, or exposure-times depict identically the same details. Since the application of photography to such delineations it has become a question whether it is worth while to spend the necessary time and pains upon *drawings* of such subjects as photography is fitted to deal with.

It is certain that a photograph, properly taken, will show many features of comets, nebulae and the Milky Way which the eye cannot grasp as well, or even at all. Thus the "photographic" Milky Way must always be different from the "visual" one, and future researches may well be directed solely to the former. The recent photographs of Comet *Swift* (April, 1892), of Comet *b* (July, 1893) and of Comet *Brooks* (October, 1893) taken at the LICK Observatory, by Professors BARNARD and HUSSEY, exhibit several features in the fainter portions of these comets which are not visible to the eye and which have not been seen in earlier comets. Yet it is very possible, and even probable, that these features may have been present in former bright comets. For the tails of comets, then, we shall always prefer photographs to drawings. The case is entirely the same for nebulae. Why should it be different for the Milky Way? Photographs of the Milky Way do, in fact, show some features which the eye cannot see at all (and hence duplicate negatives should be taken for verification) and they show very many others which the eye sees, indeed, but not with sufficient sharpness to allow of a satisfactory delineation. The photograph has, however, errors of its own which are of the same general nature as the errors of the human retina; and it takes account of one part of the spectrum only.*

A considerable part of the luminous background of the Milky Way, as seen with the naked eye, is due to the fact that the eye cannot separate the different stars which really exist in the sky and whose images, therefore, overlap on the retina. Moreover, the eye cannot be rigidly fixed on one area, but leaves it and returns to it continuously; and hence the persistence of vision contributes also to the formation of the luminous background which we (apparently) see. A considerable part of this background has, therefore, no real existence, but is a strictly subjective appearance, and must, consequently, be different to different eyes. When a telescope is employed to view the Milky Way, phenomena of the same general nature occur. A telescope has a greater separating power than the eye, and more stars are separately seen; but those which are not individually shown produce the effects just described. The larger the telescope the greater the separating power and the larger the number of stars which form their individual images, and the fewer stars, therefore,

* See *Publications A. S. P.*, Vol. I, page 119.

whose images overlap and go to form a luminous background. The larger apertures will, therefore, give the greatest number of individual stars, the least amount of false background and the truest representation of the Milky Way as it veritably is.

Each different telescope, aperture and magnifying-power produces a different appearance. If, now, we employ a telescope to form a photographic image on a sensitive film (which film always has a sensible thickness), a luminous background will be produced, due to the causes named above and to others peculiar to photography. All the stars separated by the telescope will form definite images on the front surface of the film and will affect the particles of silver at that surface, producing photographic images. Some of the light which goes to form a given image will pass through the film and be reflected from the back surface of the glass plate, and returning through the film will produce *halation* rings around the central image. The light which thus passes through the film, twice, meets particles of silver in its course. It tends to decompose all the particles which it meets. Some of it is reflected from their surfaces also and irregularly dispersed, and many particles of silver which are not in the *direct* path of any ray are, nevertheless, affected by these irregularly scattered rays. All the stars which are too close together in the sky to be separated by the photographic telescope employed will form overlapping images on the film, and will thus contribute to form a luminous, nebulous background, which is, in part, simply an instrumental defect, and not a fact of nature. The imperfect polish of the lenses or mirrors of the photographic telescope employed contribute to the same thing. The intensity of this background varies with the exposure-time, the amount of incident light, the aperture and focus, the kind of plate employed, and with the nature of the development. If the plate is not accurately in focus the instrumental defects are exaggerated. It is impossible to get rid of all instrumental shortcomings, but the most important one can be avoided. The reflections from the back surface of the glass plate which carries the sensitive film can be destroyed by suitably backing the plate either by a permanent coating or by the ingenious backing which Professor SCHAEBERLE has described in these *Publications*, Vol. IV, page 272.

It is very desirable to have complete pictures of the Milky Way as seen with the naked eye (these we now possess); with

small photographic telescopes (these are now being made at the LICK Observatory and elsewhere); and by greater instruments (these will eventually be obtained in the course of making the International Photographic Charts, and also with the BRUCE telescope of the Harvard College Observatory).

The object of all work of this kind is to determine the true nature of the Milky Way. This requires a comparison of maps made by instruments of varying power, and on a uniform plan throughout. The varying transparency of the sky at different altitudes and on different nights cannot be eliminated, but every other condition should be made as constant as possible. There is nothing new in the foregoing principles, but they seem to form a proper commentary on Mr. EASTON'S remarks as to the difficulties encountered in the problem of depicting the Milky Way.

Just as in the photographic photometry of stars it is of advantage to establish photographic magnitudes without any attempt to make them agree with visual magnitudes so in this case we shall do well to investigate the "photographic" Milky Way without any reference to the "visual" one.

After taking into account all the advantages and disadvantages of the best possible representations of the Milky Way made by the eye and made by photography, it seems to be unquestionable that the latter process is the only one which should be employed in the future. The photographic representations if made according to a fixed programme are entirely satisfactory and have a definite meaning. Drawings may be of very high value—as for example the drawings of Messrs. BOEDDICKER and EASTON, but it seems to be certain that the time, patience, skill and artistic talent lavished on such drawings can, in the present state of science, be more usefully employed. The case is analogous to that of the Nebula of *Orion*. We have every reason to be grateful to the observers who have given us accurate drawings like those of LASSELL, BOND, ROSSE, but the magnificent photograph of Mr. COMMON has a higher authority than everything that preceded it. In the same way suitable photographs of the Milky Way will, I think, take the place of even the best drawings of it, such as the maps of Mr. EASTON.

There is an important economy possible in photography which is obvious enough but which should not pass without mention. If a pair of exactly similar cameras are mounted side by side, it

is practicable to obtain *duplicate* plates for the final chart with a *single* exposure. Other cameras giving larger (or smaller) areas can readily be exposed at the same time. A *cluster* of cameras can be pointed about as readily as a pair, and five or six plates can be obtained with a single exposure. Each camera and each plate will record a different galaxy (just as each eye sees its own Milky Way) but the very differences due to variations in light, focus, definition, plate, etc., are precisely what are needed to give the data for a thorough study, provided they are *simultaneously* recorded on the different plates.

The remarks made by Mr. EASTON upon the various theories of the Milky Way which have been enunciated are conceived in the true scientific spirit. It is certain that the galaxy presents streams and aggregations of stars which most strongly suggest definite laws of formation. It is entirely proper to point out the laws which are suggested. But it is of the first importance to insist that these results are, for the present, only suggestions. It seems to be quite possible that in the future the real laws in question may be discovered. The work of the present is, first and most important, to obtain the necessary data; and second, to discuss these data as completely as possible, but always with the explicit statement that, for the present, the data are insufficient.

Previous researches on the Milky Way from the time of PTOLEMY until the present day are described by Mr. EASTON in a succinct but valuable historical note. The description of PTOLEMY is given in full, with explanatory notes, in Appendix I. Appendix II is devoted to a description of the galaxy by Dr. KLEIN. Appendix III is a reprint of HOUZEAU's determination of the brilliancy of various regions, with comparisons between these determinations and those of Mr. EASTON. A detailed description of the Milky Way (comprising 20 folio pages) is next presented by the author from his own observations, which is followed by a catalogue referring to the principal features—bright areas, streams, and dark spaces. Finally the atlas contains three detailed charts of the Milky Way, a general chart on a smaller scale, and an analytical map, in which the outlines of the various streams, etc., are laid down, but no shading given.

In concluding this brief notice, it is only just to point out that this work of Mr. EASTON's appears to be a model of what such a work should be, and to say that it is very fortunate for science that we now have two such monographs of the Milky Way as those of BOEDDICKER and EASTON.

It seems to be also entirely proper to emphasize my personal opinion as to future researches of the kind, which is, that such future researches should be carried on by photography ; and this for two reasons—*first*, the labor of skilled and faithful workers will thus be available for other investigations, *second*, the “photographic” Milky Way, as we may call it, can be mapped again and again during the time required to make a single complete drawing. It must be recollected, however, that both the drawings last mentioned were commenced and well under way before it was known how simply and satisfactorily such representations could be made by the camera.

LICK OBSERVATORY, December, 1893.

THE DISINTEGRATION OF COMETS.

BY PROFESSOR H. A. NEWTON.

[Professor H. A. NEWTON of Yale University recently delivered the following extempore address before the American Philosophical Society. It is of special interest, since no one is better qualified than Professor NEWTON to speak upon questions relating to meteors and comets.]

“I have to apologize somewhat in that I came to the rooms not expecting to speak to you. I have, however, one point which I think will interest the members of this Society if they will give me a few minutes to develop it, and that is, the force which acts on the small bodies sent off from comets and which form our shooting stars.

“There are in the comets so many questions that we cannot answer, so many curious and wonderful phenomena that are unexplained, that I am sure you will accept any explanation of any of them that seems plausible, as a matter of interest. From a comet there is continually driven off matter forming the tail, a light substance, and astronomers are agreed that the force that acts on the matter which forms the tail is a repulsive force from the Sun acting inversely as the square of the distance, the force of the repulsion being greater than that of attraction.

“Not only is this true, but different parts of that tail are acted upon by repulsive forces of different powers ; otherwise the tail

would form across the sky a single line instead of a broad, expanded mass of light such as we see. From the comet, however, there are driven off also, or there are separated, other things entirely distinct from the tail, small bodies, which are not thus driven away, which are not visible, but follow along closely in the path of the comet, and whenever the occasion comes, that is when we go through a group of them, those give us our shooting stars.

"The BIELA comet, in the period about 1840, passed near to *Jupiter*. At that time it was turned pretty sharply out of its orbit, the inclination of the orbit being turned several degrees, and the node being carried forward also several degrees, represented by several days in the time at which we crossed the path of the comet.

"After 1840 the bodies which formed the meteors that were met in 1872 and in 1885 were separated from one or other parts of the BIELA comet. I say after 1840, because if they had been separated earlier they would have given us a different radiant in the skies, the one given by the BIELA meteors of 1838. The radiant was changed, the node was changed, all to correspond to the new orbit, and these bodies could not have been turned in that way had they been before scattered, because the force that acted on them, the attraction of *Jupiter*, would have scattered the group instead of giving us that single compact group through which we passed in 1872 and 1885 in the course of four or five hours, and the bulk of them even in two hours.

"In 1872, the comet was something like 200,000,000 miles away from the bodies that we met as we passed through them on the 27th of November, giving us a brilliant shower. Thirteen years later we passed through the group again, and then we were something like 300,000,000 miles ahead of the comet. So that some of the particles leaving the comet between 1840 and 1885 had gained, and others between 1840 and 1870 had fallen behind.

"What should separate those particles? What are the forces which carried off those particles so many miles—200,000,000 miles on the one hand and 300,000,000 miles on the other in round numbers? The force that acts on them must be a force acting in one plane, that is, the plane of the orbit of the comet. Any force acting in other planes would have scattered the group, and we would not have met them as a single definite group at the times named; but if it acts in the plane, only scattering them on the plane, they would be together as we saw them.

"In that plane it must be either an impulsive force acting once or it must be a constant force acting continually. The only bodies in that plane are the comets and the Sun, and if the force is a continuous force it must be from the comet or from the Sun. It is almost inconceivable to suppose that the comet could have sent them off, either impulsively or continuously, in such a way as to give us the distance of 200,000,000 and 300,000,000 miles in the course of thirty years ; it would require far more than any velocity that we can give in our terrestrial experiments, and we have no reason to suppose that there is any such power of impulsions. Moreover, if the impulsion came from the comet they would go in all directions and their character, as being in a plane, would have been entirely lost.

"We are then thrown back on this one hypothesis, that the Sun is the source of that force. In other words, we are led to extend the idea that I gave you in the beginning, and which is accepted by astronomers, that the material which goes off from the comet, after it leaves it, is subject to a force like that of attraction but differing in its intensity. In the case of the tail, it is a repulsive force. To satisfy these conditions of separation, part in one direction and part in the other, from the comet, we must have an attraction in the one case exceeding the attraction of gravitation, and in the other an attraction less than the attraction of gravitation. In other words, these little bodies of hard matter that go off from the comet and follow very nearly in its train are acted on not in proportion to this mass like the force that acts on the planets in their orbits.

"I see no escape, myself, from this conclusion. What it means I must leave to you to decide. Our experiments make it very improbable that the attraction of matter differs in any way from proportion to the mass. It looks to me as though the more natural explanation is that, in some way, the materials which go off from the comet carry with them a load of electricity, or something of that kind, by which they have a permanent repulsion or permanent attraction sufficient to change the orbit altogether, not in kind, but in a steady change, throwing them into a new orbit with a new period, and thus scattering them.

"What that added force must be we cannot very well tell, because it differs according to the place in the orbit where the disintegration takes place. If that disintegration takes place near the Sun, it is one thing ; if it takes place near *Jupiter*, it is

another. It looks more to me as though there was a disintegration all along the line of the comet's orbit, giving us small particles with all sorts of loads of electricity and all sorts of differences of central attraction and differences of orbits, and thus they get widely scattered so as to give us the showers a long distance from the comet itself. The amount of this change would have to be something like the tenth part, possibly, or something less than that. I should think that all the phenomena could be explained by a change amounting to one-tenth of the attraction ; that is, if the small particle carries a load of electricity such as to diminish the attraction to say nine-tenths of the original attractive force of the Sun, or increase it to eleven-tenths, it will explain the phenomena.

"If that is the explanation, we come to this further conclusion of interest, that the space through which these comets move is not such that the electricity which the particle carries can be lost. Another practical point would be that, in the discussion of the separation of these comet masses that through the telescope we see going off as the comets pass the Sun, there might fairly be introduced an unknown correction of the force of central attraction."

"A MEMBER : 'Have you gentlemen, who have made a study of this very interesting subject which you have been discoursing on, arrived at any hypothesis as to what broke up the *BIELA* comet?'"

"PROF. NEWTON : 'I can only answer that a working hypothesis, in my own mind, is that a mass not surrounded by an atmosphere, coming down from the cold into a warmer region near the Sun, becomes heated up, and in that heating there is a disintegration going on. If you put the pieces of a meteorite into a vacuum and heat them you will get gases that will be something like those which are thrown off from the tail of a comet, and the comet coming down near the Sun, with the hot, scorching effect entirely undiminished by a thick atmosphere, would have pieces broken off, giving fresh surfaces. An immense amount of action of some sort follows, and those pieces would naturally go off under such excitement carrying with them, as I conceive, a load of electricity. The process goes on in almost all our comets. It is not in *Biela* alone that we see comets going off to pieces. Scores of comets have shown that same breaking up under the telescope.'"

THE DESIGN OF THE CORDOBA CATALOGUES.
EXTRACT FROM "AN ACCOUNT OF SOUTH
AMERICAN ASTRONOMY"* GIVEN AT
THE MEETING OF THE A. S. P.

BY R. H. TUCKER, JR.

The list of the catalogues of the Cordoba Observatory is an epitome of sidereal work in the southern sky. The history of their design and execution is a condensed outline of the progress of astronomy, as far as it relates to positions of the stars, for the northern sky as well. The catalogues already published represent the principal lines of work of the northern observers up to the time of the foundation of the Cordoba Observatory; and in their proper order, while later undertakings will be found to be meeting the needs of southern astronomy.

Being condensed into a period of a few years, such problems as proper motion lie entirely outside their scope, and time enough has not elapsed to accumulate the great mass of separate determinations needed for the solution of others; but this condensation itself adds to their value in other respects. It is a striking example of wise design and efficient, energetic direction. The time was ripe for the development of southern astronomy, and it was not necessary to originate means and methods to keep pace with improvements in instruments and in systems of observation, advantage could be taken of all that experience and practice had taught and the whole scheme carried forward without halt or repetition.

The main feature of the original design of Dr. GOULD was the observation of a zone catalogue of the southern sky, similar in its purpose to the Zones of BESSEL and others in the north. These zones are observed by sweeping with the meridian circle within certain narrow limits, and as many stars as possible of those encountered were taken. The result will be a catalogue of stars pretty uniformly distributed throughout the extent covered, but

* NOTE.—Included in the original paper were discussions of other lines of work at Cordoba, such as the photographs of clusters, observation of comets and asteroids and latitude and longitude determinations; also, an account was given of the expedition to observe the last solar eclipse, and descriptions of visits to the observatories of Rio Janeiro and Santiago de Chile, and of the new observatory at La Plata in the province of Buenos Ayres.

no limiting magnitude can be given to which the catalogue will be absolutely complete. The places will be accurate enough for the first determination, by comparison, of the position of a comet or asteroid, and it is pretty certain, from the formation of the catalogue, that a star near the position desired will be found among the zones observed, if any exists, as bright as the 9th magnitude.

The Zones of BESSEL cover the region between -15° and $+45^{\circ}$.

ARGELANDER extended the work from 45° to the Pole, and also from -15° to -30° .

Besides these, the catalogues of LALANDE, D'AGELET, SCHWERD, LAMONT, RÜMKER, CARRINGTON, SCHJELLERUP and the sixth volume of the *Bonn Observations* can be included among those used for the same purpose as the Cordoba Zone Catalogue. Besides the above, there are zone observations at Washington, and others less consulted.

GILLISS' observations at Santiago cover the 20° nearest the South Pole, and the Cordoba Zone Catalogue fills the field between, extending from -23° to -80° . It contains, in round numbers, 105,000 observations of 73,000 stars, and was observed in the years 1872 to 1875, being published in 1884.

Previous to the arrival and during the mounting of the meridian circle, the observations for the Uranometry were made. This was the determination of the magnitudes of all southern stars visible to the naked eye. It thus includes such stars as were given in the earliest charts designed to represent the sky; but a more exact comparison is with the Uranometries of BAYER, ARGELANDER and HEIS.

The Uranometry, published in 1879, is thus the first step in the Cordoba scheme of observation, as the earliest charts were for the northern sky. Similar work had also been done in the south previous to the founding of the Cordoba Observatory. But the Uranometria Argentina includes all the stars thus listed, and yet is independent in its execution.

Following the Uranometry and Zone Catalogue, the Argentine General Catalogue gives accurate places by means of observations often extended for one star over several years, and it includes not only all the bright stars, but all those observed by any other southern observatory previous to that time.

These are such observations as have been made, and are still

continued by the leading northern observatories. They are the foundation of modern standard star catalogues, and furnish the data for the positions given in the several almanacs. Incidental to the formation of the General Catalogue was the yearly computation of the ephemeris of the southern circumpolar stars, used for the determination of instrumental corrections, carrying out the analogy between astronomical work in the two hemispheres, even to the computation of needed ephemerides. The General Catalogue contains 145,000 observations of 32,000 stars, and was published in 1886.

Though not next in order, the scheme of united observation of all stars down to the 9th magnitude, usually referred to as that of the *Astronomische Gesellschaft*, should be taken up. That has been considered the most important astronomical undertaking of the century. For a basis it must have a complete enumeration of all the stars, to be observed by the meridian circle. This basis is furnished for the northern sky by the *Durchmusterung* of ARGELANDER. The sky has been portioned out to various observatories, Albany and Cambridge in this country being among the number, and the observations have been completed. SCHÖNFELD'S *Durchmusterung* extended the list to -23° , and this region is now under course of observation. To that is joined the Cordoba *Durchmusterung*, which already extends to -42° , and will furnish the list for southern observers. The meridian circle is already employed at Cordoba on the first section -22° to -26° , and previously had been used in the determination of the fundamental stars for this work, in combination with other observatories in the south.

Retracing the groundwork of this extensive design, the Gesellschaft system may be considered as the secondary triangulation, including all stars to the 9th magnitude. It is to be based upon the complete reconnaissance furnished by the *Durchmusterung*. The General Catalogue forms the primary triangulation, with much other work equally exact.

The Uranometry is the first reconnaissance, not necessarily giving anything that was not known with some degree of exactness before, but completing and improving the first efforts in the same direction. The needs of astronomy could not await their completion, and the zone catalogues, north and south, have fairly well filled the field, which will be completely covered when the last of the Gesellschaft scheme shall be concluded.

January 27, 1894.

ASTRONOMICAL OBSERVATIONS.

Made by TORVALD KÖHL, at Odder, Denmark, in the year 1893.

VARIABLE STARS.

*Z Cygni.**

Jan.	10.	$Z > b.$	Oct.	2.	$Z = b.$
	21.	<i>id.</i>		10.	<i>id.</i>
Aug.	1.	$Z = a.$		13.	$Z = a.$
	9.	$Z = b.$		23.	<i>id.</i>
	20.	$Z \text{ a little } > b.$	Nov.	2.	<i>id.</i>
Sept.	2.	$Z > b.$		6.	<i>id.</i>
	10.	<i>id.</i>	Dec.	2.	$Z < c.$
	26.	<i>id.</i>		12.	$Z = e.$

*X² Cygni.*

Jan.	10.	$X^2 = g.$	Oct.	30.	<i>id.</i>
Sept.	2.	$X^2 = f.$	Nov.	2.	<i>id.</i>
	10.	<i>id.</i>		4.	$X^2 > X^1.$
Oct.	2.	$X^2 \text{ a little } > a.$		6.	<i>id.</i>
	10.	$X^2 \text{ almost } = B.$		10.	<i>id.</i>
	13.	$X^2 = B.$	Dec.	2.	$X^2 \text{ a little } < X^1.$
	23.	$X^2 \text{ almost } = X^1.$		12.	$X^1 > X^2 > A.$

* Vide Publications A. S. P., No. 22, page 62.

SHOOTING STARS.

No.	Time (P. M.).			Beginning.		End.		Mag.	Note.
	<i>h.</i>	<i>m.</i>	<i>s.</i>	<i>o</i>	<i>o</i>	<i>o</i>	<i>o</i>		
1	August 9,	10	5	0	25+39	22+31	1		
2	"		7	40	358+10	353+ 4	1		
3	"		10	30	357+34	350+28	3		fast.
4	"		16	20	346+10	339+ 3	2		
5	"		20	40	16+30	10+24	1		train.
6	"		27	10	22+39	16+32	2		
7	"		32	0	302+10	302+ 2	3		
8	"		33	45	355+35	348+28	3		
9	"		36	10	297+59	272+41	2½		id.
10	"		41	40	283+19	270+ 7	1		id.
11	"	11	1	30	34+67	30+72	1		id.
12	"		20	30	314+34	304+26	3		id.
13	"		23	0	29+29	41+31	2		slow.
14	"		24	40	338÷ 6	327÷15	1		train.
15	"		37	10	323+19	313+ 6	3		fast.
16	"		37	30	3+37	14+35	2		
17	"		40	30	16+15	8+ 5	1		
18	"		42	0	322+29	301+20	2		
19	"		44	30	352+18	357+20	3		
20	"		47	50	326+ 6	319÷ 4	2½		train, yellow
21	"		51	40	5+11	5+11	1		stationary.
22	"		57	5	330+55	312+44	1		train.
23	"	10,	9	58 10	58+83	180+84	2		
24	"		10	2 10	319+50	300+34	3		
25	"		4	30	25+67	350+69	2		
26	"		18	0	286+55	282+37	1		id.
27	"		20	0	337÷ 3	331÷ 9	1		
28	"		24	20	273+28	266+10	1		id.
29	"		26	30	337+48	314+22	2½		id., yellow
30	"		28	20	302+48	287+33	1		
31	"		32	30	3+14	359+ 4	1		
32	"		40	0	310+10	302+ 2	2		
33	"		41	40	324+15	329+ 7	3		

SHOOTING STARS—Continued.

No.	Time (P. M.).			Beginning.	End.	Mag.	Note.
	<i>h.</i>	<i>m.</i>	<i>s.</i>	$\begin{smallmatrix} \circ & \circ \end{smallmatrix}$	$\begin{smallmatrix} \circ & \circ \end{smallmatrix}$		
34	August 10,	43	40	333+25	324+16	3	
35	"	54	20	10+20	6+14	2	train.
36	"	10	55 30	27+41	20+47	1	train, slow.
37	"	58	0	317+28	308+16	1	train.
38	"	11	0 0	328+ 5	322÷ 3	2	
39	"	5	0	339+48	325+36	2	
40	"	8	0	306÷ 8	299÷ 15	2	id.
41	"	9	50	345+24	336+10	2	id.
42	"	11	50	312+10	303+ 0	2	id.
43	"	28	10	338+16	334+ 8	1	
44	"	32	0	322+35	314+23	2	
45	"	37	40	333÷ 1	326÷ 9	2	
46	"	40	20	336+36	323+22	3	
47	"	43	40	51+32	53+27	1	
48	"	46	10	345+30	337+21	4	
49	"	49	40	311÷ 12	307÷ 17	2	
50	"	51	40	355+61	324+50	1	id.
51	"	53	0	348+41	333+33	3	id.
52	"	58	30	339+62	315+55	1	id.
53	"	11, 10	0 50	356+13	338÷ 8	♀	fireball.
54	"	5	10	307+39	289+24	3	fast.
55	"	14	40	302+17	293+13	1	crossing <i>Allair</i> .
56	"	19	0	9+49	2+44	2	train.
57	"	21	50	315+18	310+10	4	
58	"	23	40	320+20	310+ 8	2	id.
59	"	32	19	325÷ 13	321÷ 17	1	
60	"	32	30	7+39	356+27	1	id.
61	"	41	0	27+42	24+34	1	id.
62	"	44	30	328+37	315+23	4	
63	"	44	40	300+50	293+35	3	
64	"	59	40	312+24	305+10	3	
65	"	11	10 0	345+53	325+40	2	id.
66	"	14	30	339+ 6	338÷ 5	4	

SHOOTING STARS—Continued.

No.	Time (P. M.).	Beginning.	End.	Mag.	Note.
	<i>h.</i> <i>m.</i> <i>s.</i>	<i>°</i> <i>'</i>	<i>°</i> <i>'</i>		
67	August 11, 23 5	347+5	339÷4	3	
68	" 27 0	21+30	14+15	2½	id.
69	" 29 20	350+42	335+40	2	
70	" 42 0	8+40	2+35	1	id.
71	" 11 47 0	332+34	321+21	2	train.
72	" 50 0	8+40	0+32	2	
73	" 53 30	325÷18	321÷22	2½	
74	" 55 50	301+18	296+1	2	id.
75	" 12, 9 58 0	20+66	40+60	3	
76	Nov. 6, 8 38 0	320+30	298+14	1	slow, undulated.

No. 34. This meteor has also been observed at Fredericia, where the end point was fixed : $344^{\circ}+28^{\circ}$ (2d mag., train).

No. 37. Was observed at Fredericia in the position : $354^{\circ}+42^{\circ}\longrightarrow 1^{\circ}+44^{\circ}$ (3d mag.).

No. 42. End point observed at Fredericia in the position : $316^{\circ}+9^{\circ}$ (1st mag., train).

No. 58. Was observed at Copenhagen in the position : $252^{\circ}+24^{\circ}\longrightarrow 245^{\circ}+11^{\circ}$ (2d mag.).

No. 60. Was observed at Copenhagen in the position : $255^{\circ}+73^{\circ}\longrightarrow 230^{\circ}+45^{\circ}$ (2d mag., orange, train stationary for a moment).

The five meteors named have given the following results :

No.	Beginning.			End.			Real Length of the path.	Observer.
	<i>h</i>	λ	ϕ	<i>h</i>	λ	ϕ	β	
		<i>°</i> <i>'</i>	<i>°</i> <i>'</i>		<i>°</i> <i>'</i>	<i>°</i> <i>'</i>		
34	..	...	...	102	1 30	55 16	...	{ TORVALD KÖHL
37	74	2 5	55 36	44	2 30	55 36	37	{ JOHN PETERSEN
42	..	...	...	81	2 29	54 52	...	id.
58	110	1 30	55 17	82	1 55	55 8	38	{ TORVALD KÖHL
60	109	0 22	56 5	74	1 2	55 50	55	{ T. ANDERBERG
								id.

** *h* and β indicate kilometres ; λ is W. Long. from Copenhagen ; ϕ is N. Lat. ; *h* is the altitude of the meteor above the Earth's surface.

Odder is situated in $2^{\circ} 25'$ W. Long. from Copenhagen, and $55^{\circ} 58'$ N. Lat. Fredericia has the geographical position $2^{\circ} 50'$ W. Long. from Copenhagen, and $55^{\circ} 34'$ N. Lat.

On the following dates : January 19 ; March 13, 25 ; April 6, 17 ; May 13 ; July 17 ; September 14, 27 ; October 2, 22, and November 1, fireballs have been observed at different places in Denmark. In my journal, about fireballs, going back to the year 1875, it is to be noticed *that the days 27 and 28 September indicate a period of frequent meteors.*

* * * * * * *

The accompanying* map of Denmark contains sixty projections of meteors seen from various stations in this country. The five above-mentioned calculated meteors are in the map identical with the Nos. 56, 57, 58, 59, 60.

* * * * * * *

The little observatory in the garden of the Real-School at Odder, has been altered in the past year, so that the telescope-room, which formerly stood on the ground, has been lifted three meters, on account of the erection of new buildings in the immediate neighborhood.

LIST OF EARTHQUAKES IN CALIFORNIA FOR THE YEAR 1893.

COMPILED BY C. D. PERRINE.

The following list gives the dates and places of occurrence of earthquakes in California (including also a number outside of the State), compiled from observations at Mount Hamilton, Mills College, Alameda and Carson City (Nev.), and reports received at the LICK Observatory both by letter and newspaper. It is a continuation of such reports printed in Vol. II, page 74; Vol. III, page 247, and Vol. V, page 127, of these *Publications*.

A more complete account will be published as a bulletin by the United States Geological Survey. The dates are civil dates. The times are Pacific Standard (*i. e.*, railroad times.)

Roman numerals enclosed in parenthesis indicate the intensity on the ROSSI-FOREL scale.

* It has not been practicable to reproduce this map.

LIST OF EARTHQUAKE SHOCKS.

- January 13, 1^h 2^m 22^s A. M. Mount Hamilton. (V)
 January 24, 9^h 40^m P. M.; 25, 1^h A. M. Winters.
 February 21, Fairfield, 8^h 15^m P. M.; Suisun, 8^h 16^m P. M.; Dixon,
 8^h 20^m P. M.
 March 3, Grass Valley, 6^h 15^m A. M.
 March 6. Umatilla (Oregon).
 March 27, Santa Rosa, 11^h 30^m P. M.
 March 30, Independence, 10^h 30^m P. M.; Lone Pine.
 March 31, Vacaville, 2^h 30^m A. M.
 April 4, Mojave, 11^h 40^m A. M.; Saugus; San Bernardino, 11^h
 40^m A. M.; Los Angeles, 11^h 48^m A. M.; San Diego, 11^h 42^m
 A. M., Santa Ana, 11^h 45^m A. M.; Newhall.
 April 6-8. Las Lunas, Belin and other Rio Grande River towns
 in New Mexico.
 April 13, Hydesville, 5^h A. M.; Eureka, 5^h 10^m A. M.
 April 21, San Rafael, 11^h 15^m P. M.
 May 18, Santa Barbara, 4^h 35^m P. M.; Saticoy, 4^h 36^m P. M.; San
 Pedro, 4^h 35^m P. M.; Santa Ana, 4^h 25^m P. M., Lompoc, 4^h
 P. M.; Ventura, 4^h 35^m P. M.; Oakland, 10^h 3^m A. M.; Carson
 City (Nev.), 2^h 55^m P. M.; Los Angeles, 4^h 30^m P. M.
 June 1, Santa Barbara, 4^h A. M.
 June 6, San Francisco, 9^h 25^m A. M.; Alameda, 9^h 30^m A. M.
 June 18, Santa Rosa, 5^h A. M.
 June 30, Vallejo, 5^h 30^m A. M.; Mare Island; San Rafael, 5^h 35^m
 A. M.; Petaluma, 5^h A. M.
 July 12. Albuquerque (N. M.)
 July 30, San Francisco, 1^h 30^m A. M.; Oakland, 1^h 30^m A. M.
 August 5, Mount Hamilton, 9^h 16^m P. M. (I)
 August 9, San Francisco, 1^h 15^m A. M.; Sacramento; San Diego,
 11^h 2^m A. M. and 4^h 7^m P. M.; San Rafael, 1^h 13^m A. M.; Ala-
 meda, 1^h 15^m A. M.; Petaluma, 1^h 10^m A. M.; Santa Rosa, 1^h
 12^m A. M.; Sonoma, 1^h A. M.; Healdsburg, 1^h 10^m A. M.
 August 12, Mills College, 12^h 50^m P. M.; Alameda.
 August 14, Toutle River, near Mt. St. Helens (Wash.), 5^h
 7^m A. M.
 September 1, Gilroy, 11^h 20^m P. M.
 September 2. San Jose.
 September 6, Redding, 8^h 22^m A. M.
 September 7. Las Lunas and central New Mexico.

September 28, Mount Hamilton, 6^h 20^m A. M. (II)

October 15, Santa Cruz, 5^h A. M.

November 1 to 7. Guadalajara (Mexico), earthquakes for ten days prior to November 7. Disturbances in the States of Oaxaca, Puebla, Guerrero, Morelos and Jalisco. The Colima volcano in a violent state of eruption.

November 21, Capistrano, 7^h 48^m P. M.

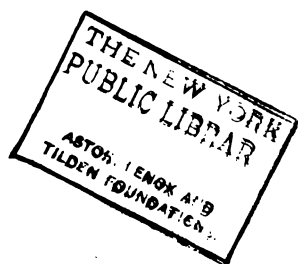
December 12, Lakeport, 3^h A. M.; Ukiah, 3^h 15^m A. M.

December 17, Ontario, 10^h 50^m P. M.; Riverside, 10^h 40^m P. M.

Four earthquakes are reported as occurring at St. Augustine Island, Alaska, in the summer.

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THE NEW DUDLEY OBSERVATORY.



NOTICES FROM THE LICK OBSERVATORY.

PREPARED BY MEMBERS OF THE STAFF.

EARTHQUAKE OF NOVEMBER 6 (OCTOBER 6?), 1711.

IN MALLET'S list of earthquakes (*Report B. A. A. S.*, 1852, p. 114), there is a reference to a light earthquake shock felt on October 6, 1711, in the neighborhood of Paris. MALLET's note is copied from *Mémoires R. Acad. Sci.*, Paris, 1711, p. 7, where the date is given as October 6, without particulars. No other reference to this earthquake can be found in the library of the LICK Observatory. The *Mémoires* of the Duc de ST. SIMON (A. D. 1711, Chap. XII) refer to this earthquake, but give a date one month later. As the date is of some importance, and as more details are given by ST. SIMON, I have copied the paragraph, and leave the date to be settled by those who have access to large libraries.

E. S. H.

"On sentit sur les huit heures,* du 6 novembre, à Paris et à Versailles, un tremblement de terre si léger qu'assez peu de gens s'en aperçurent. Il fut très-sensible vers la Touraine et le Poitou en quelques endroits, le même jour et à la même heure, en Saxe et dans quelques villes d'Allemagne voisines."

TWENTY-INCH EQUATORIAL FOR THE OBSERVATORY
OF MANILA.

MR. G. N. SAEGMÜLLER of Washington has just completed the mounting for a 20-inch telescope for Manila. The moving parts weigh about 7500 pounds. A pull of 4 pounds on the hand-wheels will move the instrument in Right Ascension and a trifle less will move it in Declination. A pull of one-eighth of a pound at the eye-end will move it in either direction.

* Du soir. E. S. H.

By means of finding-circles at the south end of the pier, the telescope can be quickly set to the nearest minute of Right Ascension and to 5' in Declination. The Right Ascension finding-circle is driven by a sidereal clock, so that the setting is to the Right Ascension of the object sought, not to the hour-angle.

Mr. SÆGMÜLLER has lately made mountings for the 12-inch equatorials at the United States Naval Observatory, Washington, and for the LADD Observatory, Providence; and for the 9-inch equatorial of the Catholic University of America, Washington. The last edition of his printed catalogue gives plates and general descriptions of his mountings.

E. S. H.

LARGE REFRACTOR FOR THE OBSERVATORY OF MEUDON.

A great refractor has just been finished and placed in position for Dr. JANSSEN at Meudon. It is a combined photographic and visual telescope. The two lenses were made by the celebrated HENRY Brothers, of the Paris Observatory. The mounting is by GAUTHIER, of Paris. Both lenses will be mounted in the same tube, which is *square* and of steel. The visual objective is 82 cm. (32.3 English inches) in diameter, while the photographic objective is 63 cm. (24.8 English inches) diameter. Both lenses are of the same focal length, 17 meters (669 English inches.) The large objective will be the guiding part of the instrument when used for photography. This great telescope is housed in the ruins of the old royal palace, a part of the ruins serving as the tower for the great dome, which dome is 20 meters (66 English feet) in diameter and weighs some 60 or 80 tons. The dome is to be moved by a gas engine of 12 horse-power. The observing chair is attached to the dome and moves with it. All the fine circles are to be read from the eye-end by means of electric lights, the electricity for which is generated by an 8-horse-power engine half a mile distant, in what was formerly the royal stables.—*Scientific American*, November 18, 1893.

SUMMARY OF RAINFALL AND TEMPERATURE OBSERVATIONS
FROM SEPT., 1880, TO NOV., 1893, INCLUSIVE, AT THE
LICK OBSERVATORY [COMPILED BY C. D. PERRINE].

MONTH.	Average Rainfall.	TEMPERATURE.					
		Average Maximum.	Average Minimum.	Mean.	Highest Maximum on Record.	Lowest Minimum on Record.	Average Daily Range.
July	0.00	79.2	60.6	69.9	94	36	18.6
August	0.01	80.0	63.3	71.7	96	39	16.7
September	0.30	73.5	56.1	64.8	93	30	17.4
October	1.44	62.9	46.9	54.9	88	25	16.0
November	2.56	56.2	41.5	48.9	88	26	14.7
December	9.06	49.7	36.3	43.0	72	17	13.4
January	3.85	48.4	36.6	42.5	72	14	11.8
February	4.31	47.9	33.6	40.7	74	12	14.3
March	5.62	51.0	37.0	44.0	80	18	14.0
April	3.68	58.5	37.1	47.8	82	21	21.4
May	1.76	64.0	46.9	55.5	90	28	17.1
June	0.59	70.0	51.1	60.5	92	31	18.9
Annual	33.18	61.8	45.6	53.7	. . .	. . .	16.2

BRIGHT METEOR (?) IN NORTH CAROLINA, DECEMBER 20, 1893.

Hon. E. S. MARTIN, of Wilmington, sends the following newspaper clipping relative to a bright meteor (?) seen in Raleigh on the morning of December 20th.

"There was a remarkable spectacle in the sky, a little south of east, early this morning, which greatly frightened the negroes. About 3 o'clock a luminous mass, about the size of a barrel, was seen in the sky, at an altitude of about 20 degrees above the horizon. It was dazzlingly white, and from its upper portion a shaft of light rose to a great height. As daylight came this shaft assumed a zigzag form. It was 6.15 o'clock when the luminous object disappeared, appearing to sink. Negroes in the country ran to the houses of white people and told them of the spectacle in the sky, and confessed their fright. Many people in this city saw the strange sight."

MISSING MAPS OF ARGELANDER'S *DURCHMUSTERUNG* WANTED.

The LICK Observatory desires to purchase maps Nos. 2, 3, 5 and 6 of ARGELANDER'S *Durchmusterung*. E. S. H.

UNIVERSAL TIME.

The scheme for making the standard time of all countries of the world depend on the Greenwich meridian has advanced another step. Since November 1 last, the time used on the Italian railways, and probably, therefore, throughout the country, is that of a meridian exactly 1 hour east of Greenwich.

The following summary gives the present state of things with reference to the Universal Time question :

- (1) England, Belgium and Holland use Greenwich time.
- (2) In Germany the legal time since May 1, 1893, is exactly 1 hour fast on Greenwich. Italy uses similar time; and a bill is before the Austrian Parliament whose purpose is to make such time legal in Austria. In Sweden the time in use is almost exactly 1 hour fast on Greenwich.
- (3) In Australia the Postal and Telegraph Conference, lately held, passed a resolution that it would be advisable to use one time throughout the Continent, that time being 9 hours fast on Greenwich.
- (4) In the United States and Canada the territories are divided into hourly zones, in each of which the time in use is respectively 4, 5, 6, 7 or 8 hours slow on Greenwich time.

At the same time that the above change was made on the Italian railways it was decided to adopt the 24-hour system of time notation, the next hour after noon being called 13. It is strange that this plan was proposed so long as twenty-six years ago, and only now been adopted. We have lately had brought to our notice a circular issued in the Paris Exhibition of 1867, in which this plan was proposed and specimen railway time-tables made in accordance with it. A writer in *L'Astronomie* asks how long will France have to wait for this reform? We re-echo the enquiry with relation to England.—From *The Observatory* for December, 1893.

[*Addendum.* In addition to what is given above it may be noted that the standard on all the oceans is practically Greenwich time. The Greenwich standard—with a change of the integral hour only—prevails nearly continuously, therefore, from 1 hour east of Greenwich (Austria, Germany, Italy) across the Atlantic, the continent of North America (where the system has been in use since November, 1883) and across the Pacific also. E. S. H.]

AWARD OF THE ARAGO MEDAL TO PROFESSORS HALL AND
BARNARD.

The Paris Academy of Sciences has awarded the ARAGO Medal to Professor HALL of Washington and to Professor BARNARD of the LICK Observatory, for their discoveries of new satellites to the planets *Mars* (HALL, 1877) and *Jupiter* (BARNARD, 1892), respectively.

E. S. H.

THE NEW DUDLEY OBSERVATORY.

By the courtesy of the editor of the *Scientific American* we are able to give, in the present number, a view of the new DUDLEY Observatory at Albany. See these *Publications*, Vol. V, pp. 100, 226.

CONTRIBUTIONS TO THE LITERATURE OF *NOVA AURIGÆ*.

Contributions to the literature of the new star of 1892, *Nova Aurigæ*, have continued to be numerous, even in the last half of the year 1893. Professor VOGEL's comprehensive paper of sixty quarto pages is most interesting and suggestive. It comprises the Potsdam observations in full; a scholarly review of all the important observations made at other observatories; criticisms of the theories propounded by others for explaining the *Nova*; and, finally, the statement of his own theory.

Professor VOGEL's paper called out several counter-criticisms, notably those of Professor SEELIGER and Dr. BELOPOLSKY. It was our purpose to review all these papers in this number of the *Publications*. But the discovery of another "new star" in *Norma*, whose spectrum is apparently identical with that of *Nova Aurigæ*, renders it unwise to make any comments until the complete observations of *Nova Normæ* are published. The discovery of two such stars with identical spectra will probably modify very essentially the views now generally held. We shall, however, print herewith a translation of Professor VOGEL's concluding paragraphs containing the statement of his theory.

W. W. C.

MT. HAMILTON, January 1, 1894.

VOGEL'S THEORY OF *NOVA AURIGÆ*.

[Translated by Mrs. ELIZABETH B. CAMPBELL from *Über den neuen Stern im Fuhrmann*, Royal Academy of Sciences, Berlin.]

"The view that the *Nova* is to be explained by the encounter of one heavenly body with several bodies urged itself upon me after the first observations, and this idea has, in the course of time, been continually strengthened by further observations. Here the question whether the probability for a meeting of such a nature among heavenly bodies is not too slight a one, gives rise to doubts, to be sure. Yet these doubts seem wholly removed by the consideration that, according to the KANT-LAPLACE theory of the origin of our solar system, a greater body can indeed scarcely be thought of without companions; and it seems altogether surprising that among all the hypotheses of new stars this proposition, without being investigated further, has been allowed to go unheeded. If one supposes that a body, whose mass is of the order of our Sun's mass, should suddenly come close to a solar system similar to ours, whose central star, through gradual cooling off, had lost its light, a tremendous disturbance would be caused thereby, and collisions with single members of the system, and appearances of light consequent thereon, would be inevitable.

"The body which in the composite spectrum of the *Nova* has shown the continuous spectrum with absorption bands, and which, as known, travels through space with a velocity of about 400 miles (per second), might have come close to a system whose motion did not deviate from ordinary proportions, for whose direction of motion no special supposition would need to be made.

"By passing close to a greater or several smaller bodies of the system, perhaps also by direct collision with smaller bodies, the star entering the system is suddenly transformed into a state of intense heat. At the time of the spectroscopic observations, the body found itself in a part of the supposed solar system which was filled up the more thickly with diminutive bodies. These, on account of the near approach and partial collisions, have, next in order, assumed the state of intense heat which the surface and atmosphere of the penetrating body must have had, as shown by the continuous spectrum with absorption lines, displaced far toward the violet. They have in this way received a very great degree of heat and a greater or less velocity, to which

the spectrum with bright lines owes its origin. They have thus produced an effect similar to the particles of cosmic cloud in SEELIGER'S theory, except that this essential difference exists, that the motion of the little bodies is regulated by the central body, so that while they keep up a continual stream toward the penetrating body, they cannot move upon it from all directions.

"With this explanation, it becomes clear why the bright lines were broadened, pushed to one side, and faded. Also, there is no longer anything strange in the fact that the bright lines, which showed the least displacement (the violet edges of the bands) did not coincide with the center of the dark lines, but bespoke a slight motion in space which possibly was not very different from that of the supposed solar system.

"By reason of the unavoidable tidal disturbances and the eruptions consequent thereon, increase of heat in the atmosphere of the central body and the larger bodies of the system followed, which, even if not so intense that the surface of the body presented a higher temperature than its atmosphere, would in any case have given a spectrum with special bright lines, whatever was next to be expected from the outside heating of the smaller bodies by falling upon the penetrating body. Thus, in a simple way, does the intensity-maximum of the bright hydrogen lines explain itself, which signifies a slight movement in space, and which in the beginning possessed the greatest intensity.

"Moreover, for the second intensity-maximum which maintained itself a long time, and for the third maximum of only passing occurrence in the bright hydrogen lines, yes, even for the fine bright lines which occur in the dark hydrogen lines, explanations can be found (under the supposition that these last are not to be considered as reversed appearances, under which, by the way, they can be explained by none of the before mentioned hypotheses) in the foundation of the suppositions, for whose probability in so disturbed a system sufficient points of support have been given.

"More particularly I would adduce, that the irregularities which were observed in the measures of the D lines, inasmuch as the displacement of the middle of the line in the star spectrum from its normal position was found to be less than in the hydrogen lines (HUGGINS, BECKER), as well as similar observations of the finer chromosphere lines (CAMPBELL), are here given explanation, since in the spectrum resulting from different bodies the same

lines would not need to enter in. Further, I call attention to the fact that the second illumination of the *Nova* in the autumn of 1892 can be attributed to a meeting of the body hastening through the supposed solar system with a single outside member of the same; and I notice still further, that indeed the surest proof of the correctness of the views here expounded would be given, if the changes of the wave-lengths of the bright lines in the spectrum now visible would allow themselves to be proven with greater certainty as CAMPBELL's observations indicate them, which would allow us to assume an orbital motion.

"However, I will no longer lose myself in details, since it was my purpose in the main, only to show: that the probability for the meeting of a body traveling about in space with an orderly system of bodies is none too slight, since against the supposition of a planetary system among the fixed stars there is nothing to be urged; and that through the supposition of such a system, in which a body which moves with the abnormal velocity of 400 to 450 miles could have remained for weeks, yes, months (since, for example, it would need fully five months to traverse our solar system) the most important of the observed appearances of the *Nova* can find a natural explanation."

THE NEW STAR IN *NORMA*.

Brief reference was made in the November number of these *Publications*, page 225, to the discovery of a new star in the southern sky. In response to Professor PICKERING's telegram to the observatories, announcing the discovery, Professor KAPTEYN suggested that the star was almost without doubt identical with a 9.2 magnitude star shown on five Cape of Good Hope plates, exposed in the years 1887-1890. Fortunately Professor PICKERING was able to remove all doubt on this point; and in case the star remains bright a few months we shall probably obtain considerable information which will be exceedingly valuable in the study of new stars. Professor PICKERING points out the fact that the main features of this star's spectrum are identical with those of *Nova Aurigæ*'s February, 1892 spectrum. If now the bright lines in *Nova Normæ*'s spectrum should be found to be multiple, as was the case in *Nova Aurigæ*'s spectrum, we could safely attribute an identical origin to the two stars. Further, many of the *special* theories propounded by various

spectroscopists to explain *Nova Aurigæ*'s spectrum would necessarily give way to the more *general* theories.

When this star is on the meridian of Mt. Hamilton, its altitude above the horizon is only about $2\frac{1}{2}^{\circ}$. While observations of it will be attempted here in February and the following months, they cannot be successful in more than a very limited degree.

We quote in the following article Professor PICKERING'S account of the discovery, as published in the *Astronomische Nachrichten*.
W. W. C.

A NEW STAR IN *NORMA*.

"A new star appeared in the constellation *Norma* during last summer. It was discovered by Mrs. M. FLEMING on October 26, when examining a photograph of the spectra of the stars in its vicinity. The photograph was taken on July 10, 1893, at the Arequipa station of this observatory by Professor SOLON J. BAILEY. The spectrum appears to be identical with that of the new star which appeared in *Auriga* in December, 1891. Comparing the spectra of the two stars taken with nearly the same dispersion, about a dozen lines are visible in each and are identical in wave-length. The hydrogen line $H\beta$ which is bright in both stars is more intense in the star in *Norma* than in that in *Auriga*. It is in fact more intense in the former star than any other line in the spectrum, while the $H\gamma$ line is generally the strongest in *Nova Aurigæ*.

"A photograph taken June 21, 1893, showed the spectra of stars of the 10th magnitude, but no trace of the new star was visible upon this plate, although it covered the same region and was in other respects like that taken on July 10. Photographic charts of the same region taken on June 6, June 10, July 21, 1889; May 16, May 16, June 10, June 23, June 23, 1891; May 7 and May 27, 1893, show no image of this object, although stars of the 14th magnitude are visible upon some of them. We may therefore conclude that the star appeared within ten days of the first of July, and that previously it was either invisible or extremely faint.

"The position was found by comparing the ends of the hydrogen lines $H\gamma$ and $H\delta$ which are bright in this star, with the corresponding dark lines in the adjacent stars *Cord. G. C.* 20940 and 21006. They give the approximate mean position for 1900 in Right Ascension $15^h 22^m 12^s$, Declination $-50^{\circ} 13'.8$. A more

accurate position can be found if photographic charts can be taken showing this star. Professor BAILEY has been notified of this discovery and if the star is still bright enough, he will doubtless obtain photographs showing its position and spectrum.

"The similarity of these two new stars is interesting; first, since it has proved a means of discovering one of these objects, and secondly, because if confirmed by other new stars it will indicate that they belong to a distinct class resembling each other in composition or physical condition.

"The star was approximately of the 7th magnitude photographically on July 10, since it was about equal to *Cord. G. C.* 20910, magnitude 6.9, whose spectrum is of the second type. The nearest catalogue stars are *Cord. G. C.* 20940, magnitude 8, which has a spectrum of the first type, and *Cord. G. C.* 20926, magnitude $8\frac{3}{4}$, which has a spectrum of the second type. The new star lies nearly midway between these two."

EDWARD C. PICKERING,

Director Harvard College Observatory.

CAMBRIDGE, MASS., U. S. A., Nov. 9, 1893.

THE DISCOVERY OF ASTEROIDS IN 1893.

Thirty-four minor planets were discovered in 1893: eight by WOLF of Heidelberg, twenty-five by CHARLOIS of Nice and one by BORELLY of Marseilles. WOLF and CHARLOIS used the photographic method, first successfully employed by WOLF in 1891.

The number of minor planets now known is about 380. The rate of their discovery by photography in the past two years has been so rapid that the computers are no longer able to make the computations for determining their orbits properly. We believe that Dr. WOLF has for this reason desisted from making further special search for them. Dr. WOLF's decision is to be commended. The cosmical questions with which the asteroids may be connected will certainly be nearest solution if the computations for those already discovered are kept up to date.

W. W. C.

CONFERRING OF THE DEGREE OF DOCTOR OF MATHEMATICAL SCIENCES UPON MISS DOROTHEA KLUMPKE.

Mlle. KLUMPKE, who has just gained the degree of Doctor of Mathematical Sciences at the Sorbonne, is the first lady who has obtained that distinction. The full title of her thesis was "Con-

tribution à l'étude des anneaux de Saturne," and the following is a translation from *La Nature* of the complimentary terms in which M. DARBOUX addressed the gifted authoress in granting her the degree: "You have occupied yourself with one of the most interesting questions in astronomy. The great names of GALILEO, HUYGHENS, CASSINI and LAPLACE, without speaking of those of my illustrious colleagues and friends, are connected with the history of each of the great advances in the attractive but difficult theory of the rings of Saturn. Your work is not a slight contribution to the subject, and it places you in an honorable position among the ladies who have devoted themselves to the study of mathematics. During the last century Mlle. MARIE AGNESI gave us a work on the differential and integral calculus. Since then SOPHIA GERMAIN, as remarkable for her literary and philosophic talent as for her mathematical faculties, was held in esteem by the great geometers who honored our country at the beginning of this century. And but a few years ago the Academy of Sciences, on the report of a commission in which I had the honor to take part, awarded one of its best prizes to Mme. KOWALEWSKA, placing her name by the side of those of EULER and LAGRANGE in the history of discoveries relating to the theory of the movement of a solid body around a fixed point. In your turn you have entered upon your career. We know that for some years you have devoted yourself with great zeal and success to investigations connected with the star chart. Your thesis, which you have prepared according to our course of higher mathematics, with an assiduity that we could not ignore, is the first that a lady has presented and successfully sustained before our faculty to obtain the degree of Doctor of Mathematical Sciences. You have worked in a deserving manner, and the faculty has unanimously decided to declare you worthy of the grade of Doctor."—From *Nature* for December 28, 1893.

PHOTOGRAPHIC MAP OF THE NORMAL SOLAR SPECTRUM
[MADE BY PROFESSOR H. A. ROWLAND].

"This series of photographs of the solar spectrum has been made in the Physical Laboratory of the JOHNS HOPKINS University. Several concave gratings, of 6 inches diameter and $21\frac{1}{2}$ feet radius, having 10,000 or 20,000 lines to the inch, were used for the purpose. The process of making this map is the well-

known ROWLAND method, and is based on the property of the concave grating as discovered by Professor ROWLAND: this property is that the spectrum, as photographed in any given order, is normal and of the same scale throughout. The focus remains automatically adjusted so that one has only to move the instrument to the part of the spectrum required, absorb the overlying spectra and put in the photographic plate. The negatives enlarged have been selected from many hundreds taken from different gratings, though three gratings were finally selected for the work. The negatives from any given order of spectrum are measured from one standard line to another on a dividing engine, so that the constant of the dividing engine is known. The scale is made by ruling on a piece of French plate glass having a coating of blackened collodio-chloride. The negatives are then clamped to the scale firmly, after being adjusted into position by the standards. They are next put in the enlarging apparatus; and the whole is enlarged from $2\frac{1}{2}$ to possibly 4 times, so as to make the scale of the map about 3 times that of ANGSTRÖM's map. The positives thus made are then figured, and negatives are made from them by contact.

"In the negatives so far examined the scale has been placed within less than $\frac{1}{10}$ ANGSTRÖM division or $\frac{1}{100000}$ wave-length of its true position.

"As to the definition of the spectra, much is lost in the enlargement, not from want of definition in the enlarging lens, a 25 x 21 inch DALLMEYER rapid rectilinear, but from the radical defect of photographic processes: for when one brings out the fine doubles in which the streak of light in the centre is very faint, he loses many of the fainter lines. The original negatives show E, and even finer lines like those at wave-lengths 5276.1 and 5914.3 plainly double; but there is little hope of showing this on the map. The atmospheric line just outside of one of the D lines also nearly merges into it, although in the original negative it is widely sundered from it.

"As to comparison with other maps of the spectrum made by measurement and drawing, it may be said that no comparison is possible. The photograph is the work of the sunlight itself, and the user of this map has the solar spectrum itself before him, and not a distorted drawing full of errors of wave-length and of in-

tensity. The superiority is so great that there is no possibility for comparison.

“The following is a list of the plates :

a from wave-length 3000 to 3330	f from wave-length 4850 to 5350
b “ “ “ 3270 to 3730	g “ “ “ 5250 to 5750
c “ “ “ 3670 to 4130	h “ “ “ 5650 to 6150
d “ “ “ 4050 to 4550	i “ “ “ 6050 to 6550
e “ “ “ 4450 to 4950	j “ “ “ 6450 to 6950

“The plates will be delivered in Baltimore or New York or will be sent by express or mail, securely packed, at the charge and risk of the purchaser, at the following *net* prices : Set of ten plates, wave-length 3000 to 6950, \$20.00 ; single plates, \$2.50. Should any extra plates continuing the spectrum in either direction be published, subscribers can have them at \$2.00 each.

EXTRA PLATES.

“Two plates have been made of the B and D lines. The latter are 3 inches apart, and the former has an extent of about 24 inches. Two enlargements of some of the carbon bands from the arc electric light have also been made. They show the wonderful structure of these bands, each containing many hundred lines, each one of which is a close double or, in some cases, a triple. These plates will be sold for \$2.25 each, unmounted, or for \$2.50 mounted on cloth.”

Orders should be addressed to The Johns Hopkins Press, Baltimore, Maryland, U. S. A.

PHOTOGRAPHING THE CORONA.

The *Observatory* for December credits Professor GEORGE E. HALE of Chicago with the statement that the attempt to photograph the corona without an eclipse, which was made on Pike's Peak last summer without success, will be continued this summer on Mount Etna with the assistance of Professor TACCHINI. The unsuccessful results on Pike's Peak were attributed to the impurities in the atmosphere at that time. It is expected that the blue Italian sky will be more favorable.

W. W. C.

THE VARIATION OF THE INTENSITY OF GRAVITY AT DIFFERENT POINTS OF THE EARTH'S SURFACE.

Commandant DEFFORGES, of the geographical service of the French army, has been engaged since 1884 in making pendulum determinations of the intensity of gravity at different points of western Europe and northern Africa. His work comprises forty-one determinations at thirty-five different stations. He has recently been able to connect his results and those of twelve other European geodesists into one homogenous system, and to draw from them some most remarkable conclusions.

CLAIRAUT has given a formula for determining the force of gravity at any point whose latitude is given. It has long been known that at some stations the values obtained from observations are greater than those given by CLAIRAUT's law, while at other stations they are less. These anomalies of gravity have been attributed either to corresponding anomalies in the figure of the Earth, or to the insufficiency of the formulæ for reducing to sea level, or to the unequal distribution of mass in the Earth's crust, or finally, in most cases, to the insufficiency or to the errors of observation. Commandant DEFFORGES' deductions from these anomalies are as follows*:

"From all of our measures and all of the old measures which can be united with them with safety, there result some very plain facts, henceforth incontestable, and which can be generalized. They can be stated briefly, thus:

"Gravity is distributed very unequally over the surface of the globe.

"The law of CLAIRAUT, true in general, is nearly everywhere marked by appreciable anomalies.

"Gravity, upon the shores of different seas, presents some small anomalies; constant for any given shore and consequently characteristic.

"On the islands there is considerable excess of gravity.

"On the continents the inverse is true, and the defect seems to increase proportionally to the altitude and the distance from the ocean.

"We give, by way of illustration, this table of these anomalies of gravity upon a line which starts at Spitzberg, traverses Great

* Translated from *Comptes Rendus (de l'Académie des Sciences)* for July 24, 1893.

Britain, France, the Mediterranean Sea, and ends at Biskra (in Africa):

NAMES OF STATIONS.	Altitudes.*	Residuals. Obs'd-Comp'd	NAMES OF STATIONS.	Altitudes.*	Residuals. Obs'd-Comp'd
	m.			m.	
Spitzberg	6	+88	Figeae	223	-60
Unst.	9	+55	Marseille	61	+14
Porstoy	29	+52	Bastia	20	+93
Leith	21	+35	Corte	605	+34
Clifton	104	+14	Ajaccio	6	+41
Arbury Hitt	225	+32	Philippville	20	+41
London	25	+15	Col des Oliviers	420	-13
Greenwich	48	0	Constantine	655	-64
Shanklin Farm.	74	+7	Ouled Rhamoun	687	-21
Dunkerque	20	+5	Aïn Yagout	890	-90
Lihons	106	-3	Batna	1050	-145
Paris	60	-18	El Kantara	525	-143
Lyon	286	-10	Biskra	137	-100
Clermont.	406	-63			

*The altitudes are expressed in metres, and the residuals are expressed as so many hundred-thousandths of a metre. Thus, at Biskra the observed effect of gravity in one second is an acceleration of 9.79698 and the computed is 9.79798 metres, giving the residual—100.—[TRANSLATOR.]

“The residuals (anomalies), positive in Spitzberg, Scotland and Corsica, become negative on the French and Algerian continents. The residuals on the continents increase with the altitude and the distance from the sea.

“It must be noticed also that from Shetland to the Mediterranean, the real surface of the ellipsoid does not differ at any point, according to CLARKE, from the theoretical surface by more than 6.4 metres. We cannot, therefore, attribute the anomalies in gravity to anomalies in the figure of the Earth.

“It is from geology that we must ask an explanation of these irregularities.

“The English measures in India, connected with ours at Kew and Greenwich, furnish a splendid confirmation of the laws determined in Europe and Africa, in Great Britain, France and Algiers.”

These results derive their great interest from the fact that they are precisely the reverse of what would naturally be expected. If the intensity were *less* on small islands and on seashores than in the interiors of continents no comment would be called for, since the density of water is very much less than that of the Earth's crust.

Commandant DEFFORGES determined the intensity of gravity on Mt. Hamilton, in October, 1893. He uses a "reversible invertable" pendulum invented and manufactured by himself, by which it is claimed that many errors hitherto very troublesome have been wholly eliminated, and that the results for relative gravity are certainly correct within one part in one hundred thousand.

W. W. C.

THE NEW NOTATION FOR THE HYDROGEN LINES.

A rational system of notation is at last to be given to the fourteen lines in the hydrogen spectrum. Heretofore, three incomplete systems have been used. Professor VOGEL has now suggested (in *Astronomische Nachrichten*, No. 3198) a simple nomenclature which will undoubtedly be employed hereafter by all spectroscopists. The new and old nomenclatures are given in the second and third columns of the following table. The first column contains the approximate wave-lengths of the lines, in tenth-metres :

Wave-Length.	New Name.	Old Name.
6563	H _{α}	C or H _{α}
4861	H _{β}	F or H _{β}
4341	H _{γ}	H _{γ} , often called G
4102	H _{δ}	h or H _{δ}
3969	H _{ϵ}	H or H _{ϵ}
3889	H _{ζ}	α
3836	H _{η}	β
3798	H _{θ}	γ
3771	H _{ι}	δ
3750	H _{χ}	ϵ
3734	H _{λ}	ζ
3722	H _{μ}	η
3712	H _{ν}	θ
3704	H _{ξ}	ι

W. W. C.

ELEVATION OF THE LICK OBSERVATORY ABOVE SEA, AS
DETERMINED BY OFFICERS OF THE UNITED STATES
COAST AND GEODETIC SURVEY.

[NOTE.—I have the permission of the Chief of the Survey to print the following important memorandum relating to determinations of the elevation of Mount Hamilton. EDWARD S. HOLDEN.]

EDWARD S. HOLDEN.]

COMPUTING DIVISION,
U. S. COAST AND GEODETIC SURVEY, }
March 9, 1887.

Mr. B. A. COLONNA.

Assistant in Charge of Office and Topography.

Dear Sir: In accordance with your promise made to Assistant GEORGE DAVIDSON, I herewith communicate the height of Mount Hamilton (top of [12-inch] Dome of the LICK Observatory) together with the several other surrounding heights from which it had been determined. The computation and the adjustment had just been completed by Mr. DOOLITTLE. The zenith distances measured between the objects are shown by arrows on the accompanying sketch. [Omitted.] The observations were made by Messrs. MARR, MORSE, HILL, and DICKINS in 1883-84-85, parties in charge of Assistant DAVIDSON.

The resulting heights above the half-tide level of the Pacific depend upon the known heights of Mt. Diablo and Mt. Tamalpais* and are as follows:

	Metres.
Mt. Diablo	1173.10
Mt. Tamalpais	789.39
Sierra Morena	735.41
Rocky Mound	427.64
Red Hill**	55.60
Mt. Bache (Loma Prieta)***	1158.56
Mt. Toro	1083.19
Santa Ana ****	1103.74
Macho	1244.97
And Mt. Hamilton (top of dome [12-inch])	1295.31
“ “ “ “ “ “ “ “	feet 4249. 7

The individual results for Mt. Hamilton are as follows:

					Metres.
Mt. Hamilton, height of top of dome from				Mt. Diablo . . .	1299.18
“ “ “ “ “				Sierra Morena .	1295.19
“ “ “ “ “				Loma Prieta . .	1293.32
“ “ “ “ “				Mt. Toro . . .	1294.41
“ “ “ “ “				Mt. Santa Ana .	1206.10

* Tamalpais has not been levelled.—[DAVIDSON.]

**** Assistant CUTTS found 57.12 metres in 1852.**

中和率	“	“	“	1156.2	“	“	1854-55.
-----	---	---	---	--------	---	---	----------

“ “ “ II03.3 “ “ I854-55.

Comparing these values with the weighted* and adjusted mean, 1295.31 metres, we can estimate its probable error at ± 0.67 metres.

The various values of the resulting co-efficient of refraction for coast stations agree fairly (minimum, 0.073; maximum, 0.107, from 16 values); the mean, or 0.0867, was used in the above computation. It will be noticed that the mean value in connection with the heights of the "DAVIDSON Quadrilaterals" was 0.0725, the difference being due to the greater dryness of the interior air.

When Macho is occupied we shall obtain an additional value for the height of Mt. Hamilton.

I would suggest that Assistant DAVIDSON be authorized to transmit a copy of this to Professor HOLDEN with the request that the latter furnish the difference of elevation between the top of the dome (12-inch) and some other fixed base level on the observatory building, to which to refer the height, as well as the prospective pendulum work. Yours respectfully,

CHAS. A. SCHOTT,

Assistant in Charge of Computing Division."

ADJUSTMENT OF MARCH, 1886; SANTA ANA NOT THEN ON RECORD.
FROM OFFICE, JANUARY 23, 1887.

	ϕ	λ
Diablo-Tamalpais-Hamilton		
Diablo-Mt. Hamilton . . .	37° 20' 24."752	121° 38' 35".284
Mt. Bache-Toro-Hamilton		
Toro-Mt. Hamilton	37 20 24.753	121 38 35.284
Sierra Morena-Bache-Hamilton		
Bache-Hamilton	37 20 24.754	121 38 35.283
Sierra Morena-Bache-Hamilton		
Sierra Morena-Hamilton . . .	37 20 24.752	121 38 35.284
	37° 20' 24."753	121° 38' 35".284

MEASUREMENTS ABOUT THE 12-INCH DOME BY C. D. PERRINE.

	Feet.
Top of dome above dome floor	22.32
Dome floor to upper hall floor	3.10
Upper hall floor to marble floor	15.15

Height of top of dome above marble floor of LICK Observatory . 40.57

The shutter rises 0.92 feet above the top of the dome.

Combining the elevation of the top of dome as determined by

*Relative weight = $\frac{1}{s^2} \cdot \frac{n \cdot n_1}{n + n_1}$; where s equals distance of stations, and n n_1 , the number of days of observations at the stations.

the Coast Survey (4249.7 feet) with the above determination (40.6 feet) we obtain

Height of the marble floor of the LICK Observatory above the half-tide level of the Pacific, as determined (1883-85) by the officers of the U. S. Coast and Geodetic Survey . . . 4209.1 feet

In Vol. III of these *Publications* (1891), page 370, we find

- I. Height of the marble floor of the LICK Observatory above the S. P. R. R. tracks at broad-guage station, San José, as determined by two sets of levellings by the students of the University of California under the direction of W. G. RAYMOND, Instructor of Engineering (1887) 4120.76 feet
- II. Assumed elevation of the tracks above mean-tide of the Pacific (from several levellings by the Engineers of the S. P. R. R.) 88.7 feet

Whence,

Height of the marble floor by direct levelling from San José (1887) 4209.5 feet

It is worth while to record here

Heliostat mirror of the photoheliograph, *above* the marble floor . 5.25 feet

Top of the marble pier (in the N. E. basement of the Meridian-Circle House) which is used for pendulum determinations of gravity, *below* the marble floor 5.2 feet

DETERMINATION OF THE ERRORS OF THE MERIDIAN CIRCLE IN 1893.

The following list of some of the instrumental errors of the meridian circle shows a satisfactory degree of stability.

Although many other determinations were made, only those are included which depend on the regular set for a full night's observing; three nadir observations for level and for circle reading; and at least one of collimation, though, for part of the nights, this was observed before and after the night's work.

To this will finally be added the azimuth error, determined from three pairs of circumpolar stars.

The period embraces the last four months of 1893.

	<i>c</i> s.	<i>b</i> s.	Zenith. "
September 13	−.003	+ .665	16.81
14	.000	.658	16.56
15	+ .003	.670	16.95
20	−.013	.656	17.31

	<i>c</i> s.	<i>b</i> s.	Zenith. "
September 22	-.013	.671	16.91
25	-.006	.659	16.73
October 2	-.013	.680	17.32
3	-.003	.675	16.85
5	-.006	.660	16.91
10	-.013	.625	16.91
11	-.038	.592	16.66
12	-.026	.633	16.68
13	-.023	.654	16.32
17	+.006	.664	17.09
November 2	.000	.612	16.82
3	-.006	.642	16.63
7	-.006	.626	15.93
8	-.016	.613	15.73
13	-.003	.639	15.89
14	-.016	.631	16.14
15	.000	.653	15.50
21	-.003	.572	16.85
December 6	-.010	.579	15.94
9	-.016	.555	16.20
11	-.010	.547	17.01
19	-.013	.577	16.65
29	-.032	+.547	17.39

A preliminary discussion of some of the observations already reduced gives, for the latitude of the meridian circle,

$$+37^{\circ} 20' 25''.7$$

This has, however, to be corrected for the division error of the circle reading at the nadir. It depends entirely upon places of stars from the Berlin *Jahrbuch*. R. H. T.

THE RED STAR DM. $+36^{\circ} 4025$.

In *Publications A. S. P.*, No. 31, page 177, I stated that the red star DM. $+36^{\circ} 4025$ of SECCHI's Type III, not being in SCHEINER's compiled list of third and fourth type stars, had probably not been observed before. KRÜGER's very complete catalogue of colored stars, since received, includes this star as observed by ESPIN in 1887, who classifies it as Type IV.

W. W. C.

[In the December *Monthly Notices* R. A. S., just received, ESPIN states that he re-examined this star in August, 1893, and it was "found not to be Type IV, but Type III!!" His observations suggest variability in its light.]

JUPITER'S FIFTH SATELLITE.

By comparing his observations of *Jupiter's* fifth satellite, made 1893, September 15th, with those made 1892, September 10th, Dr. BARNARD has found (*Astronomical Journal*, No. 304,) that the period of revolution of the satellite is

$$11^{\text{h}} 57^{\text{m}} 22^{\text{s}}.56.$$

This value will probably not be changed, after several years' observations, by more than one or two-tenths of a second. The other elements of the orbit are not yet known. M. TISSERAND has pointed out (*Comptes Rendus*, Vol. CXVII, page 1024) that if the orbit is not a circle some of the elliptic elements will vary rapidly, for the reason that *Jupiter* differs greatly from a spherical form. The excess of matter in *Jupiter's* equatorial regions will powerfully perturb the motion of the small and near satellite. Thus the *peri-jove* will move from west to east no less than 882° in one year—that is, the point of the orbit nearest to the planet will make a complete revolution in about five months. The eccentricity of the orbit is quite small, however, and TISSERAND'S predicted motion of the *peri-jove* cannot be verified until a long series of observations has been made.

E. S. H.

DEATH OF PROFESSOR RUDOLF WOLF.

RUDOLF WOLF, the eminent Swiss astronomer, died December 6th at the Zurich Observatory, of which he had been director since 1855.

Professor WOLF'S scientific interests embraced the very wide fields of mathematics, physics, geodesy and astronomy. His original researches along these lines were very fruitful, and his several volumes on the history of these subjects are mines of concise and accurate information, to which working astronomers have constant recourse.

It was Dr. WOLF who developed and established the periodic law of the Sun spots and placed solar observations upon a scientific basis.

The friends of this modest and conscientious investigator have

offered numerous testimonials to his simple and charming personal character. An excellent portrait of Professor WOLF may be found in *Himmel und Erde*, January, 1894, page 190.

W. W. C.

MEDALLION OF JAMES LICK.

The plate given in No. 33 is copied from a negative by Mr. A. L. COLTON from the medallion of Mr. LICK described in these *Publications*, Volume V, page 228.

AWARD OF THE JANSSEN PRIZE TO DR. LANGLEY.

The JANSSEN Prize of the Paris Academy of Sciences has been awarded to Dr. S. P. LANGLEY, Director of the Smithsonian Institution of Washington, for his researches on the distribution of the heat in the normal solar spectrum and to the influence exerted on this distribution by the solar and terrestrial atmosphere respectively.

E. S. H.

NEW NEBULA.

A faint nebula was found while charting faint stars with the 36-inch telescope on January 25th.

The approximate place for 1894.0 is

A. R. $1^h 58^m 40^s$

Decl. $2^\circ 14' N$

The place for 1860, Epoch of the N. G. C., is

A. R. $1^h 56^m 55^s$

N. P. D. $87^\circ 56'$

It is somewhat elongated, with but little condensation.

R. H. T.

PHOTOGRAPHY OF LIGHT OF SHORT WAVE-LENGTH [BY DR. VICTOR SCHUMANN].

"An account of Dr. VICTOR SCHUMANN's successes in photographing rays of very short wave-lengths is given in No. 50 of the *Naturwissenschaftliche Rundschau*. These successes are entirely due to the elimination of absorption by the material used in the prisms and lenses, and, what is specially noteworthy, of the layers of air intervening between the luminous source and the plate used for photographing the spectrum. This elimination

has resulted in the exhaustive exploration of the hitherto doubtful ultra-violet region between 231.4 and $185.2\mu\mu$ and the annexation of the region down to $100\mu\mu$ to the known spectrum. For this it was necessary to get rid of the absorption due to the film of gelatine in which the sensitive silver salt was imbedded, and this was accomplished by the substitution of a pure silver bromide plate. The camera, the spectroscopic apparatus and the spark tube were all connected together and exhausted. The very first exposure on the hydrogen spectrum showed that the known radiations of that gas only represent a portion of its total radiance. The newly-traced portion turned out to be extremely rich in lines, with a maximum at about $162\mu\mu$, and consisted of fifteen groups of lines disposed pretty evenly, containing altogether about 600 lines, with intensities decreasing from the maximum in both directions, rapidly at first and then very gradually. The wave-lengths of these lines are as yet undetermined. Provisionally, that of the smallest wave-length recorded is estimated at $100\mu\mu$. The spectra of aluminium, cadmium, cobalt and other metals end at about $170\mu\mu$. A layer of normal air 1^{mm} in thickness appears capable of absorbing all radiation of smaller wave-length than that. Dry gelatine absorbs eagerly all wave-lengths beyond $217\mu\mu$. Quartz is not suitable for prisms and lenses, and white fluor-spar is, so far, the only material that answers all requirements. The accurate determination of the new wave-lengths, the further investigation of the absorption due to air and the further extension of the ultra-violet region are the problems which Herr SCHUMANN is now working at."—Extract from *Nature* of January 11, 1894.

THEORY OF PHOTOGRAPHIC PHOTOMETRY.

In the *Monthly Notices* of the Royal Astronomical Society (Vol. 54, page 65), Captain ABNEY gives the results of his recent experiments in photographic photometry. Many experiments before 1889 had led Captain ABNEY to the conclusion (*Report* B. A. A. S., 1889, P. 487), that "for any ordinary light, intensity and exposure are interchangeable" except when the light is very feeble. The same conclusion is stated in *Nature*, October 10, 1889, page 584, as follows: "The deposit of silver made by different intensities of light varies directly as the intensity of light acting—this, of course, within such limits that reversal of the

image is not commenced and that the film is not at any part exhausted of the silver salt that can be reduced." All our experiments at the LICK Observatory since 1888 have led to a different conclusion.*

It is, therefore, very gratifying to us that Captain ABNEY's experiments confirm our own. His conclusions are that the chemical action is *not*, generally, proportional to the exposure for a constant light (on bromide plates); or, to take a special case, that "*the sum of a large number of small (short) exposures was not equal to the same exposure given at one time.*" These conclusions have an important bearing on the theory of photographic photometry in general and on the practical matter of determining the magnitudes of stars by measures of their images on negative plates in particular.

E. S. H.

THE SYSTEM OF 61 CYGNI.

Dr. WILSING of Potsdam has recently announced to the Berlin Academy of Sciences that he has detected a periodic variation of the distance between the two bright components of the star 61 *Cygni*. In the process of reducing several hundred photographic observations, taken in 1890-93 for determining the parallax of this star (one of the nearest to our system), he found that the distance between the two components varied from $20''.8$ to $21''.1$ and back to $20''.8$ in about twenty-two months. He is inclined to attribute this relative motion to the presence in that system of one or more bodies which cannot be seen with our large telescopes.

W. W. C.

* E. S. HOLDEN, in *Bulletin Congrès Astrophotographique*, Vol. I, page 297, August 15, 1889, and LICK Observatory Reports on the Eclipses of January and of December, 1889. S. W. BURNHAM, in LICK Observatory Report of the Eclipse of December, 1889, pages 8 and 35. J. M. SCHAEFER, *op. cit.*, page 35; and Terrestrial Atmospheric Absorption of the Photographic Rays of Light, page 3, equation (1). A. O. LEUSCHNER, in *Publications A. S. P.*, Vol. II, page 7 (see especially page 9 and the paragraph in italics, page 14).

MINUTES OF THE MEETING OF THE BOARD OF DIRECTORS
HELD AT THE CHABOT OBSERVATORY,
JANUARY 27, 1894.

President MOLERA in the chair. A quorum was present. The minutes of the last meeting were approved.

Professor J. M. SCHAEFERLE was elected to life membership.

The following new members were duly elected :

LIST OF MEMBERS ELECTED JANUARY 27, 1894.*

Mr. R. G. AITKEN	{	University of the Pacific, Col- lege Park, Cal.
Mr. FRANCIS D. BILLINGE	{	270 Broadway, New York, N. Y.
Mrs. MARY LYNDE CRAIG		434 Bartlett Street, S. F., Cal.
Mr. JACOB Z. DAVIS*		408 Post Street, S. F., Cal.
Mrs. JACOB Z. DAVIS*		408 Post Street, S. F., Cal.
Miss KATHARINE FLAVAN	{	2014 Van Ness Avenue, S. F., Cal.
Mr. C. A. STETEFELDT, M. E.	{	967 Alice Street, Oakland, Cal.
Mr. L. WOLTERS DORF	{	360 Ashland Boulevard, Chi- cago, Ill.

Adjourned.

MINUTES OF THE MEETING OF THE ASTRONOMICAL SOCIETY
OF THE PACIFIC, HELD AT THE CHABOT OBSERV-
ATORY, IN OAKLAND, JANUARY 27, 1894.

President MOLERA in the chair. The minutes of the last meeting were approved. The list of new members elected at the Directors' meeting was read to the meeting.

It was *voted* that the thanks of the Society be returned to the officers of the School Department and to Mr. BURCKHALTER for the use of the CHABOT Observatory.

A committee to nominate a list of eleven Directors and Committee on Publication, to be voted for at the annual meeting, was appointed, as follows: Messrs. CHAS. S. CUSHING (Chairman), ALLEN H. BABCOCK, J. COSTA, Dr. J. CALLANDREAU and JOSEPH D. GRANT.

A committee to audit the accounts of the Treasurer, and to report at the annual meeting, was appointed, as follows: Messrs. E. K. CHAPMAN (Chairman), J. A. LIGHTHIPE and EDWARD LANDE.

The following papers were presented :

- I. The Occultation of Saturn, May 25, 1893, by Mr. JOHN TEBBUTT, F. R. A. S., of Windsor, N. S. W.

* A star signifies life membership.

2. Considerations on the Methods of Representing the Milky Way ; with a Review of the Atlas of the Milky Way by Mr. C. EASTON ; by Mr. E. S. HOLDEN, of LICK Observatory.
3. South American Astronomy, by Mr. R. H. TUCKER, Jr., of LICK Observatory.
4. Note on Types of Stellar Spectra (illustrated by lantern slides), by Mr. A. O. LEUSCHNER, of the University of California.
5. On Double Star Orbits, by Mr. A. O. LEUSCHNER, of the University of California.
6. The Harvest Moon, by Mr. O. E. HARMON, of Chehalis, Washington.
7. Astronomical Observations in 1893, by Mr. TORVALD KÖHL, of Odder, Denmark.

The Chairman then introduced Mr. R. H. TUCKER, JR., of the LICK Observatory, who gave an account of astronomical work done in South America, at the Observatories of Rio de Janeiro and Cordoba.

Mr. A. O. LEUSCHNER, of the University of California, read papers on Double Star Orbits and Types of Stellar Spectra.

At the close of the meeting the equatorial telescope was placed at the disposal of the members.

Adjourned.

OFFICERS OF THE SOCIETY.

E. J. MOLERA (40 California Street, S. F.), ... *President*
 WM. M. PIERSON (Mills Building, S. F.),
 W. J. HUSSEY (Leland Stanford Jr. University, Palo Alto, Cal.), } *Vice-Presidents*
 W. W. CAMPBELL (LICK Observatory), ... *Secretary*
 F. R. ZIEL (410 California Street, S. F.), ... *Secretary and Treasurer*

Board of Directors—Messrs. ALVORD, BURCKHALTER, CAMPBELL, HOLDEN, HUSSEY, McCONNELL, MOLERA, PIERSON, SCHAEBERLE, VON GELDERN, ZIEL.

Finance Committee—Messrs. PIERSON, McCONNELL, ZIEL.

Committee on Publication—Messrs. HOLDEN, CAMPBELL, YALE.

Library Committee—Messrs. PIERSON, VON GELDERN, McCONNELL.

Committee on the Comet-Medal—Messrs. HOLDEN (*ex-officio*), SCHAEBERLE, BURCKHALTER.

OFFICERS OF THE CHICAGO SECTION.

Executive Committee—RUTHVEN W. PIKE.

OFFICERS OF THE MEXICAN SECTION.

Executive Committee—A. ANGUIANO (Chairman), CAMILO GONZALEZ, FRANCISCO RODRIGUEZ REV, AGUSTIN ARAGON.

NOTICE.

The attention of new members is called to Article VIII of the By-Laws, which provides that the annual subscription, paid on election, covers the *calendar* year only. Subsequent annual payments are due on January 1st of each succeeding calendar year. This rule is necessary in order to make our book-keeping as simple as possible. Dues sent by mail should be directed to Astronomical Society of the Pacific, 819 Market Street, San Francisco.

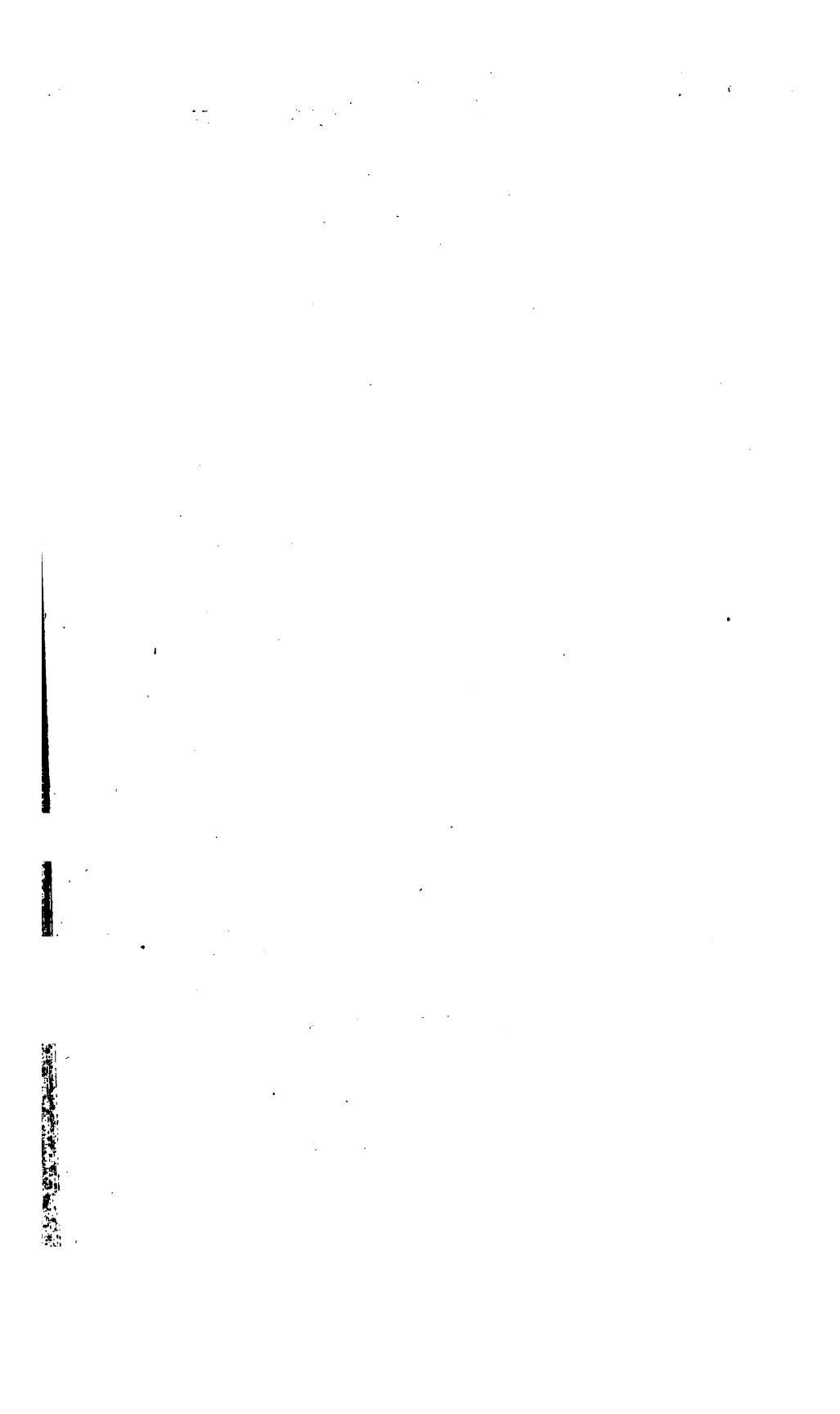
It is intended that each member of the Society shall receive a copy of each one of the *Publications* for the year in which he was elected to membership and for all subsequent years. If there have been (unfortunately) any omissions in this matter, it is requested that the Secretaries be at once notified, in order that the missing numbers may be supplied. Members are requested to preserve the copies of the *Publications* of the Society as sent to them. Once each year a title-page and contents of the preceding numbers will also be sent to the members, who can then bind the numbers together into a volume. Complete volumes for past years will also be supplied, to members only, so far as the stock in hand is sufficient, on the payment of two dollars to either of the Secretaries. Any non-resident member within the United States can obtain books from the Society's library by sending his library card with ten cents in stamps to the Secretary A. S. P., 819 Market Street, San Francisco, who will return the book and the card.

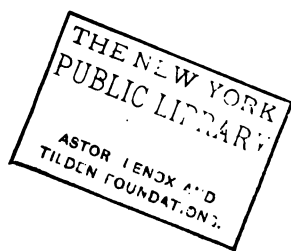
The Committee on Publication desires to say that the order in which papers are printed in the *Publications* is decided simply by convenience. In a general way, those papers are printed first which are earliest accepted for publication. It is not possible to send proof sheets of papers to be printed to authors whose residence is not within the United States. The responsibility for the views expressed in the papers printed rests with the writers, and is not assumed by the Society itself.

The titles of papers for reading should be communicated to either of the Secretaries as early as possible, as well as any changes in addresses. The Secretary in San Francisco will send to any member of the Society suitable stationery stamped with the seal of the Society, at cost price, as follows: a block of letter paper, 40 cents; of note paper, 25 cents; a package of envelopes, 25 cents. These prices include postage, and should be remitted by money-order or in U. S. postage stamps. The sendings are at the risk of the member.

Those members who propose to attend any or all of the meetings at Mount Hamilton during the summer should communicate with "The Secretary Astronomical Society of the Pacific" at the rooms of the Society, 819 Market Street, San Francisco, in order that arrangements may be made for transportation, lodging, etc.









NO. 2.

CORONA OF APRIL, 1893.

PUBLICATIONS
OF THE
Astronomical Society of the Pacific.

VOL. VI. SAN FRANCISCO, CALIFORNIA, MARCH 31, 1894. No. 35.

ADDRESS OF THE RETIRING PRESIDENT OF THE
SOCIETY, AT THE SIXTH ANNUAL
MEETING, MARCH 31, 1894.

BY E. J. MOLERA.

Following the custom of the Society, I will give you an account of the state of affairs of the Association for the year terminating to-night, adding a brief synopsis of the principal astronomical events occurring during the same period.

At the beginning of the present year we had a total membership of 481 members, and 8 having been elected at our last meeting, we have at present 489 members, 61 of whom are life members. With a little effort on the part of each one of the members of the Society there is no reason why the membership should not increase considerably.

Our financial condition continues to be satisfactory, as the report of the Treasurer has shown to you.

At each of our meetings during the past year very valuable papers have been read, and the privileges granted to members by the LICK and CHABOT observatories, made them doubly interesting.

The *Publications* have kept the high standard that has characterized them from the beginning—the matter they contain, as well as their illustrations and printing, being of the best class. The thanks of the Society are due to the Publication Committee. All the other committees have also discharged the duties entrusted to them in a most satisfactory manner.

Owing to the financial depression prevailing everywhere in the United States, the San Francisco Observatory Committee

did not consider it opportune to ask for contributions to build an observatory on the site already granted in the Golden Gate Park. It is to be hoped that, during the coming year, financial circumstances will sufficiently improve for the committee to obtain the necessary funds. New and important observatories are being built constantly; San Francisco should not any longer remain without one.

I would recommend that the members of the Society should be asked to report to the Secretary what astronomical instruments they possess, and what is the special work they pursue, so that sections could be organized composed of such members as are working in the same field, who, by their mutual co-operation, would benefit one another, and through their reports serve the purposes of the Society. Our proximity to the LICK Observatory, and the interest the astronomers of that great institution take in our behalf, should not deter any of our members from contributing his quota of work and from reporting the same to the Society. We must always remember that ours is an association of amateur astronomers.

At our last annual meeting I had an opportunity to report to you the preparations made to observe the total eclipse of the Sun of April 16, 1893, in Chile, South America, by Professor SCHAEBERLE. The expenses of this expedition were paid by Mrs. PHOEBE HEARST, also one of our members. I have the pleasure to state, what you already know, that the expedition was completely successful. Professor SCHAEBERLE started alone, but on the day of the eclipse had the volunteer assistance of several amateur astronomers, among them Mr. WALTER GALE, of Sydney, another of the members of the Society. The weather continued to be propitious to him as in other expeditions, and he made the most of his opportunity. He obtained eight photographs, 18 x 22 inches, with the 40-feet photo-heliograph lens employed in a novel manner; eight photographs with the 6-feet telescope, and the same number with the 3-feet telescope. Astronomers agree in saying that never before have such fine photographs been obtained.* The eclipse was also observed by the Harvard College Observatory expedition, under Professor WILLIAM H. PICKERING, near the same locality in Chile. England sent two expeditions; one to Fundium, Africa, where

* The frontispiece of the present number is a reduced copy from one of them.

nine negatives were secured by Sergeant KEARNEY, R. E., and one to Para Curu, Brazil, where Mr. TAYLOR made twelve negatives—six without enlargement and six with the enlarging apparatus.

A French expedition, under the direction of M. DESLANDRES, went to Fundium, and another, under M. BIGOURDAN, went to Joel, Africa. All the expeditions had good weather and were successful in their observations.

The final results have not yet been worked out, but it can be predicted that the following facts will be established:

First—The coronal matter rotates with the Sun;

Second—There is no perceptible change in the form of the corona during the few minutes of duration of the eclipse in any one place;

Third—There is very little change, if any, in the form of the corona during the few hours' time in which the point of observation changes from one point to another on the earth.

It will be remembered that Professor SCHAEBERLE predicted the general shape of the corona, should his mechanical theory of that phenomenon be correct; of course, the nature of the problem allows only of a prediction involving the general outlines. Some of the observers have contended that the drawing published before the eclipse by Professor SCHAEBERLE, and the photographs taken by them at the time of the eclipse, do not correspond. Professor SCHAEBERLE, however, was stationed several thousand feet higher than any of the other observers, so that, aside from the advantages of the large scale of some of his inner corona-photographs, the remarkable purity of the atmosphere enabled him to photograph coronal outlines of apparently far greater extent than were obtained at lower altitudes. Contrary to the verbal reports so far published by some of the other observers, he finds that the equatorial extensions of the faint outer corona much exceeded the polar extensions.

As a result of certain studies given by these and other recent data, he has been led to the following conclusions pertaining to solar and cometary phenomena :

“First—According to mechanical principles, every incandescent liquid or gaseous body rotating in space must undergo periodical changes of—

(a) Internal pressure, and

(b) Varying surface strength in the form of zonal waves having

a general motion towards the equator of the rotating body. The alternate zones of strength and weakness form in the middle latitudes, but just before reaching the equator, the weaker zones become stronger, so that the zones of least strength are again found in a higher latitude.

Second—The variation of internal pressure is due to periodical inflow, chiefly at the equator, of cooled surface matter. The expansion of the cooled matter causes an increased pressure which in turn is made manifest in a series of eruptions which will not always be along the normals. Wherever the zone of least strength has a greater latitude than that of the submerged zone of greatest pressure, the streams of ejected matter will incline towards the nearer pole of the rotating body. When the conditions are the reverse, the inclination will be towards the equator.

Third—In all such bodies the angular velocity of the surface will vary, being least in the middle latitudes.

Fourth—These mechanical principles applied to our Sun, lead to a simple explanation of the observed facts regarding the formation, motion and periodicity of the Sun spots.

The structure of coronal matter observed during the last total eclipse of the Sun plainly indicates that this matter is apparently ejected from the Sun's interior with great velocity and is subject to the force of gravitation. By far the greater portion of the ejected matter seems to come from the Sun's spot-zone region.

Many observations go to show, and some prove, that much of the ejected matter forming the corona is carried to the very confines of the solar system. Some of this matter is captured by the Earth (and by the other larger bodies of the solar system) which thus becomes the center of certain cosmic phenomena usually attributed to purely inductive forces emanating from the Sun, but here claimed to be due to the actual transference of ponderable matter from the Sun to the Earth.

Evidently a very volatile body with very small mass will have an extensive atmosphere when heated. Such an atmosphere in space will retard the velocity of a coronal stream, the diminution of the velocity will be accompanied by an increase in density of the coronal matter even to the extent of again rendering it visible at great distances from the Sun."

The solar corona is without doubt the most imposing spectacle

in the sky. The desire to explain its cause has originated the wish of studying it without an eclipse. Professor HALE has brought to a greater degree of perfection his spectro-heliograph, which is an instrument used to photograph the Sun by monochromatic light. Professor YOUNG, Dr. BRAUN, Dr. LÖHSE, Dr. ZENGER, M. JANSSEN, M. DESLANDRES and others have tried, with variable success, to photograph the solar prominences. An attempt to obtain photographs of the corona on top of Pike's Peak, last summer, was unsuccessful. It is to be hoped that the progress made in photography and mechanical ingenuity will solve this most important problem and elucidate the observed phenomena of the Sun.

The year just past has been prolific in Sun spots, some of them, in dimension and shape, being remarkable. At the exhibit of the LICK Observatory at the Midwinter Fair in San Francisco, photographs of the magnificent Sun spot of last August and September can be seen. Sun spots were prominent January 21, February 5 and 25, with opera-glasses. In March a large and singular spot was added; in April new ones came; on August 9 there was a cluster visible with the naked eye. December 7 and 8 several could be seen with a field-glass. The theory of Sun spots, the period of their appearances, their connection with the form of the corona and the magnetic disturbances observed on the Earth, has occupied the minds of many astronomers during the present year. Their views have been abundantly explained in contributions to the astronomical publications.

Three unexpected comets appeared last year :

In January, 1893, the HOLMES' comet suddenly increased in size, becoming diffused, and the phenomenon was simultaneously observed by Dr. BARNARD at the LICK Observatory and by Dr. PALISA at Vienna. According to Dr. BARNARD, this comet was certainly a most remarkable one in many respects.

Comet *b*, 1893, was notable for its discovery being made by many amateur astronomers, with few days' difference.

Mr. W. E. SPERRA of Randolph, O., observed a comet, which he supposed to be FINLAY'S, from June 20 onwards, but he did not communicate the fact to anyone in the usual way. This was, however, the new comet. Sr. ROSO DE LUNA of Logrosan, Spain, saw the comet on the night of July 4, and immediately sent a description of it to the director of the Observatory at

Madrid; the latter published a note giving an account of the discovery. Sr. ROSO supposed the object to be a new star.

Messrs. JOHNSON and MILLER seem to have seen the comet at about 8^h 30^m P. M. of July 8; Mr. ALFRED RORDAME saw it July 8, at 10^h P. M., and immediately communicated the fact by telegraph to Dr. SWIFT of Rochester. On July 9, M. QUENISSET of the Juvisy Observatory, France, Mr. FILMER of Faversham, Kent, England, Professor BOSS of Albany, and others, saw it at about the same time.

As you have seen by the report of the Committee on the Comet-Medal it has been decided not to make any award for the discovery of Comet *b*, 1893.

Professor KEELER of the Allegheny Observatory made many observations and took several spectrum photographs of this comet. During the period of the comet's greatest brightness Professor HUSSEY took several very fine photographs with the CROCKER telescope at the LICK Observatory. Professor CAMPBELL has made numerous observations, both visually and by photographic means. According to him, the comet's spectrum gives bands and lines in the carbon and cyanogen spectra.

Dr. KAYSER has made elaborate studies of the carbon bands in the electric arc, and comparisons with the spectrum of this comet. Several other astronomers have made also valuable observations of Comet *b*.

On October 16, 1893, Comet *c*, 1893, was discovered by BROOKS, who has discovered so many others. It was photographed by that renowned astronomer, Dr. E. E. BARNARD, at different times from October 18 to November 19. The photographs are excellent. They give a complete history of the extraordinary physical changes in this comet. One of these photographs, taken on the morning of November 14, shows the trail of a magnificent meteor, which shot across the sky in the region of the comet during the exposure.

This photograph exhibits such wonderful transformations of the comet, that one is inclined to believe that about October 21 it must have come in contact with some extraneous influence that shattered and disturbed its tail to a remarkable degree.

Mr. CAMPBELL has analyzed this comet and given an account of his work in our *Publications*. Bright bands are yielded by incandescent gases which surround the nucleus and form the

head of the comet. During this year Professor SCHAEBERLE announced his theory of the mechanical origin of the phenomena observed in comets, which is one of the most interesting contributions to astronomical science.

Comparatively few observations of the planets have been published during the year, the few made having been confined mainly to *Jupiter*, probably due to the attention called by the brilliant discovery of Dr. BARNARD the year previous.

The fifth satellite of *Jupiter* has been carefully and extensively observed by him at every opportunity. The final period of this satellite will not differ more than a few hundredths of a second of time from $11^h 57^m 22.6^s$. He has also made a careful re-determination of the diameters of *Jupiter* and its polar compression with the filar micrometer of the 36-inch equatorial. The resultant values are :

Equatorial diameter, 90194 ± 56 miles.

Polar diameter, 84566 ± 75 miles.

And a polar compression of $1\frac{1}{8}\frac{1}{10}$.

The equatorial diameter was measured on 34 nights and the polar on 24 nights. The other great feature observed is the gradual fading out of its prominent red spot.

Professor WILLIAM H. PICKERING's remarkable observations of the varying shapes of the satellites made in Arequipa need confirmation. If they prove to be correct, they will materially change our ideas of the physical constitution of these satellites.

Mars has been in an unfavorable position for observations ; some results from observations made in 1892 by HOLDEN, SCHAEBERLE, BARNARD, CAMPBELL, HUSSEY and others have been published during the year. I call your attention to the fine plates of *Mars* in our *Publications* and to the splendid photographs exhibited by the LICK Observatory at the Midwinter Fair.

MM. QUÉNISSET and RUDEUX, Mr. MEE and Dr. BARNARD have made interesting observations of *Saturn*. M. QUÉNISSET gives the results of his micrometric measures :

(1). The polar compression of the planet is between $\frac{1}{8}$ and $\frac{1}{10}$.

(2). The globe is not exactly at the centre of its system of rings.

Thirty-four minor planets were discovered during the year, bringing the total number to 380. Dr. BARNARD undertook to measure the diameters of the brighter asteroids with the 36-inch

telescope. This is a work that has never been successfully accomplished with smaller instruments, and a knowledge of their diameters has been mere guess work heretofore. Following are his results up to the present time :

Ceres — 599 ± 29 miles in diameter,
Pallas — 273 ± 12 miles in diameter,
Vesta — 237 ± 15 miles in diameter.

The November meteors were far more abundant this year, according to Dr. BARNARD, than in any other previous year observed by him. Especially were they plentiful on the mornings of November 13, 14 and 15. One nearly as large as the full Moon burst on the morning of the 14th just below *Coma Berenices*. Another meteor from *Leo* shot across the sky and burst between *Zeta* and *Eta Ursa Majoris*. It left a persistent train 10° long which remained bright and straight for about five minutes.

Few new observations of the Moon have been reported, excepting those in connection with the total eclipse of the Sun of April 16 and the annular eclipse of October 9.

The Moon, however, being the nearest astronomical body to us, has the greatest interest to all astronomers, who at all times study its structure and apparent topography. It has been desired to have a good map of the Moon for a great many years. Dr. LANGLEY, in 1890, addressed a circular to many public and private observatories regarding the making of a photographic map. From the advances made in photography it would seem that such a map would be comparatively easy to be produced, the only matter to be agreed upon would be its scale. The LICK Observatory has produced excellent negatives of the Moon with its great 36-inch refractor. These negatives, taken by Professor HOLDEN and Mr. COLTON, have been very successfully enlarged by Baron ALBERT VON ROTHSCHILD, of Vienna, Mr. NIELSEN, of Copenhagen, Professor WIENEK and Dr. SPITALER, of Prague. The beautiful enlargements by ROTHSCHILD are 80 inches to the Moon's diameter; in Professor WEINEK's latest results the Moon's diameter is about 10 feet.

A great deal has been said about the utility of enlarging photographs. There cannot be brought in the enlargement any feature that was not originally in the negatives, of course. The contrast between the light and the shadows is inversely propor-

tional to the square of the enlargement which places a limit to the amount of the enlargement that a negative is susceptible to sustain. Another disadvantage in photographic enlargements is that the molecules of silver are not precipitated uniformly in the vehicle in which they are suspended, so that, when an enlargement is carried too far, instead of having compact masses of pigment, there are to be seen black masses of grains, and when fine lines exist in the originals they become very indistinct, and sometimes disappear. The last objection is the greater, but a study of the different salts of silver available for photographic purposes, and their vehicles, might lead to processes of manufacture by which this hindrance could be removed. It seems that the quicker the action of an ordinary commercial photographic dry plate or its sensitiveness, the coarser is its grain. An albumen plate sensitized with nitrate of silver is hundreds of times less sensitive than one of gelatine sensitized with bromide of the same salt; but, on the other hand, the former will allow an enlargement one hundred times greater.

For photographs of the Sun, I believe that the albumen and TAUPENOT processes of photography would give wonderful results.

However, even with the existing processes, the experiments of Baron ALBERT VON ROTHSCHILD and of Professor WEINEK have shown that an atlas on a scale of 6 feet, or even 10 feet, to the Moon's diameter, can be produced. The utility of such atlases can hardly be disputed, they would give the means of convenient examination and comparison that are so essential in order to obtain general laws and features.

Leaving the planetary system, of which our own Earth is a modest member, there is nothing, perhaps, more interesting than the *Novæ*. They are near the beginning of the line of the nebular and stellar evolution. On October 26, a new star in *Norma* was discovered by Mrs. FLEMING on a photographic plate taken at the station of the Harvard College Observatory in South America.

It is remarkable that two new stars should have been discovered in the short interval of time of their appearances, when we consider the fact that only about twelve are known to have been observed since we have had astronomical records.

This new star has great importance, inasmuch as it will confirm or disprove the theories advanced to explain their existence.

At the first appearance of the *Nova Aurigæ*, it was supposed that the collision of one or more meteorites caused the sudden appearance of the lighted body in the firmament; many objections to that supposition led many to suppose that the collision took place in a grazing way.

As I pointed out to you last year, few heavenly phenomena had attracted so much attention as the *Nova Aurigæ*. Suffice to mention that amongst the astronomers who have observed and studied the new star, are BARNARD, BURNHAM, BECKER, BÉLOPOLSKY, CAMPBELL, CREW, COPELAND, Dr. and Mrs. HUGGINS, LOCKYER, MAUNDER, PICKERING, VON GOTHARD, KONKOLY, VOGEL, SIDGREAVES, WOLF, YOUNG, and others. Professor CAMPBELL has made a particular study of this kind of heavenly bodies.

The following are some of the conclusions arrived at in regard to the new stars :

(1) It is doubtful whether the new stars are composed of one body or a system of several bodies.

(2) They are of constantly changing brilliancy, for instance, the *Nova Aurigæ*, after being first discovered by Dr. ANDERSON February 1, 1892, remained bright during February and March and passed beyond the power of the 36-inch telescope in April; in August it was found again, but it had become a star whose spectrum was nebular.

(3) In July, 1893, the *Nova Normæ* showed a spectrum of bright and dark lines exactly like the *Nova* of 1892 in February; but when its spectrum was observed at the LICK Observatory in February last it was found to have turned into a nebula !

(4) The fact that the new star of 1892 in *Aurigæ* and the new star of 1893 in *Norma*, and in all probability the new star in *Cygnus* in 1876 have run through identical courses, seems to establish their similarity of evolution.

Mr. CAMPBELL'S ideas were opposed at first by many astronomers. They however were based on numerous and well-taken observations. The final success of them points out how much better it is to make many observations and few theories, than few observations and many theories.

Dr. E. E. BARNARD has continued during the year his remarkable observations of *Nebulæ* and has taken many excellent photographs. His former achievements in photography of the Milky Way will give you an idea of the excellence of his work.

It is with great satisfaction that I record among the pleasant events of the year, that the degree of Doctor in Sciences was conferred upon him by the VANDERBILT University, Nashville, Tenn., on March 8, 1893, and together with Professor HALL was honored with the *Arago* medal, never conferred before but once.

Equally valuable observations and photographs of the spectra of different nebulae were taken during the last year by Professor CAMPBELL.

The photographic chart of the heavens is progressing favorably. Methods are already being devised to use the plates for the construction of a catalogue in the near future. The chart itself will be constructed later on. The observatories of the United States of America have not joined in this work, the reason probably being that the representatives of the United States and at least one of Germany preferred telescopes capable of covering at one exposure 25 square degrees (like the BRUCE telescope of Harvard College Observatory), instead of telescopes covering only 4 square degrees, which were adopted by the commission. It will take 11,000 plates to cover the heavens according to the plan of the International Congress, whereas 1600 plates would suffice had the telescopes been constructed on the formula of a portrait-lens.

Miss BRUCE has presented to Harvard College a photo-telescope capable of performing the work as proposed by the American astronomers, and it is confidently expected that two observatories, with as many astronomers using this telescope, will do the work as completely and possibly as quickly as eighteen astronomers with as many telescopes of the form adopted by the Paris Conference. One good result from that disagreement will be the emulation among the astronomers and the duplication of results.

The BRUCE telescope will be used by the PICKERINGS at the Cambridge Observatory and afterwards in Arequipa, South America.

Besides the remarkable work in practical astronomy, this last year has witnessed many other notable events in astronomy.

The Chicago International Exhibition brought together many excellent exhibits of observatory work. On account of the International Congress of Astronomers, some distinguished astronomers were also brought together, and during their sessions read some very interesting papers and promoted lucid and instructive discussions. Another International Congress was projected by the

managers of the Midwinter Fair in this city. Circulars were sent inviting prominent astronomers all over the world, but few accepted the invitation and, on that account, the idea of convening another congress has been abandoned.

I recommend every resident member of this Society to visit the excellent exhibit at the Midwinter Fair of the University of California and examine carefully the astronomical photographs furnished by the Harvard College and the LICK Observatories. It would be impossible for me to describe to you all that is there exhibited. The Harvard and Arequipa Observatories, besides beautiful views of their own respective observatories and educational objects, show fine views of the Southern Cross, of the solar corona, nebula, comets, the Moon, etc. A very remarkable photo-map of the solar spectrum made by Professor ROWLAND, with his concave grating, forms the most interesting feature of the admirable exhibit of the JOHN HOPKINS University.

The collection of the LICK Observatory is most complete, comprising over 150 positives on glass of the most interesting celestial objects, among which I may mention: The large 18 x 22 images of the corona of 1893 by SCHAEBERLE; the beautiful one of 1889 made by BARNARD; the *Nova* by SCHAEBERLE and CAMPBELL; Sun spots by PERRINE; different planets and clusters by HOLDEN, HUSSEY and CAMPBELL; the beautiful Moon photographs taken with the large equatorial, and the splendid photographs of the Milky Way and nebula by BARNARD; landscapes and other plates by Mr. COLTON. Amongst the photo-prints, those of Professor WEINEK, of Prague, are especially remarkable, and the enlargements made by Baron VON ROTHSCHILD of LICK Observatory negatives of the Moon, by the carbon process, are without rival for their beauty.

That interest in astronomical science has not diminished is shown by the grants made of instruments which have been recorded this year. One of the most attractive exhibits at the World's (Chicago) Exposition was the YERKES refractor, soon to be finished, with an aperture of 40 inches. The photo-telescope, donated by Miss BRUCE, the largest of its kind, is another example, as well as other telescopes like the 20-inch for Manila, Phillipine Islands, and the Meudon telescope, of an aperture of 32.3 inches.

The wonders that a single year brings to the contemplation of man are truly great, and yet improved instruments and methods,

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THE CHABOT OBSERVATORY.

From a negative by F. R. ZIEGL.

and increased observers all over the world, will eclipse what is done at the present time. What SPENSER said with true prophetic inspiration, in the sixteenth century, can be repeated to-day:

“Yet all these were, when no man did them know,
Yet have from wisest ages hidden been:
And later times things more unknowne shall show.
Why then should witlesse man so much misweene,
That nothing is but that which he hath seen?
What if within the Moone's fayre shining sphere,
What if in every other starre unseene
Of other worlds he happily should heare,
He wonder would much more: yet such to some appeare.”

THE CHABOT OBSERVATORY.

BY CHAS. BURCKHALTER.

A prominent feature of the Oakland School Department is the CHABOT Observatory, the only Observatory of its kind in the world. It was founded by a public-spirited citizen of Oakland, the late ANTHONY CHABOT, Esq., and presented to the Board of Education in trust for the city of Oakland, to be used to educate the general public and the pupils of our public schools.

The original building was erected in 1883, under the direction of Hon. J. C. GILSON, who was Superintendent of Schools at the time, and Hon. WM. H. JORDAN, member of the Board of Education, and it was due to the influence of these gentlemen that Mr. CHABOT was induced to build and equip the Observatory, and the school children of Oakland will always owe them a debt of gratitude.

About one year ago the Observatory was almost entirely reconstructed, and bears no resemblance to the old building. (See cut.)

The former building consisted essentially of a transit room, reception room and the equatorial room (14 feet in diameter) at the top of the tower, and it was reached by 4 flights of zigzag stairs of 52 steps, the entrance being through a trap-door in the floor—a most awkward arrangement. Of the old building, the

transit room remains as before, the reception room, 11 x 15, is now occupied as an office and library, and the remainder was torn away.

Upon the site of the old tower the new equatorial room, 20 x 20, has been erected, 15 feet lower than the former one, and, while not only larger and more accessible, the reduced elevation has cut out about 200 arc lights scattered for miles around from Berkeley to Alameda. That these lights are a most serious evil from an astronomical point of view, it is only necessary to mention the fact that Professor BARNARD, at the LICK Observatory, is compelled to close his window blinds to shut out the electric lights of San José, distant 13 miles in a straight line, when engaged upon faint comet work.

On the first floor, in addition to the office and transit room there is a reception room 20 x 20, the lecture room 20 x 30, vestibule 6 x 10, hat room, battery room, etc., while in the second story, besides the equatorial room there is a bed room, upper hall, and an exceedingly useful little workshop, where alterations and repairs and special apparatus are made.

The new addition was paid for by the accumulated interest and a small amount of the principal of the \$10,000 bequest of the late ANTHONY CHABOT. In about two years the deficit in the principal will be made good by the interest, and will, no doubt, be as zealously guarded by future Boards as by the present Board and its predecessors. As it stands, the Observatory has cost between \$25,000 and \$30,000, but this does not represent the time and labor of the gentlemen to whom was given the execution of the plans, which, if it had been paid for, would have greatly increased the cost.

The equipment of the Observatory is as follows :

An 8-inch equatorial telescope, with micrometer and spectro-scope, by ALVAN CLARK & SONS. The chronograph and the 4 1/8-inch transit were made by FAUTH & Co., Washington. The mean-time clock, also made by FAUTH & Co., has a gravity escapement, with 60-pound pendulum, and break-circuit arrangement. The sidereal clock was made by E. HOWARD & Co. The sidereal break-circuit chronometer was made by the Messrs. NEGUS of New York.

Correct time is furnished to the city twice daily, the mean-time clock automatically breaking an electrical circuit by which the bells in all the engine houses and the City Hall bell are rung

every day at precisely 12 o'clock noon (3 strokes) and 9 P. M. (9 strokes), Pacific Standard time; the *first* stroke indicating 12 noon and 9 P. M.

There is also a complete set of meteorological instruments, mostly from GREEN of New York and RICHARD BROTHERS of Paris, and over thirty-five observations are made and recorded daily. Complete reports are sent to the Weather Bureau every month—the Observatory being a regular volunteer station—and an abstract is furnished, also monthly, to the City Board of Health for use in its report. A seismograph is kept in constant readiness to record the direction and intensity of earthquake shocks.

The lecture room is furnished with folding opera chairs, with tablet-rests for taking notes, and nearly everything necessary for technical instruction is provided. There are two lanterns, one with an arc light for projecting spectra, the other is a first-class double stereopticon, for projecting ordinary lantern slides. The lights in this lantern are powerful spiral filament incandescent lamps, and give plenty of light for a picture 12 x 12 feet—the size shown.

The projecting lamps, hand lamps near the lantern, front hall lamps, the chandeliers and platform lights are all under the control of the teacher or lecturer, by a switch on the platform, constructed in the Observatory workshop. The switch has but a single lever, and any one or two groups of lamps can be burned or dissolved at will, and the lantern lamps can also be controlled at the lantern if the teacher is without an assistant.

The equatorial room is lighted by 6 incandescent lamps that are so arranged that they can be made to burn singly, or with any degree of brilliancy, from being barely visible in the dark to full brightness. Astronomers will appreciate this feature. There are about 50 incandescent lamps in the building, the current being on from dusk until 12 o'clock; at other times gas must be used.

The Observatory is situated in the middle of Lafayette Square, which is bounded by Tenth, Eleventh, Grove and Jefferson Streets. The use of the square was donated by the city. Its exact geographical position is Lat. $37^{\circ} 48' 5''$ north; Long. $122^{\circ} 16' 39.3''$ west from Greenwich, or in time $8^h 9^m 6.62^s$ west from Greenwich; $3^h 0^m 54.58^s$ west from Washington, the longitude having been determined telegraphically by Professor GEORGE DAVIDSON of the United States Coast and Geodetic Survey, and Assistant BURCKHALTER, in connection with Messrs. MARR and

SINCLAIR, United States Coast and Geodetic Survey, the latter gentlemen exchanging signals from the DAVIDSON Observatory, San Francisco.

The rules governing the Observatory are of the simplest character. Monday and Tuesday evenings of each week are reserved for the High and Grammar schools during school terms, and Friday evening for time observations and other work, and on all other evenings excepting Sunday, the time is devoted to the instruction of visitors and private schools. Visitors are not admitted without a visitor's card from the Director's office. This rule is necessary in order to regulate the number, and to obtain a card it is only necessary to apply at the office of the Superintendent of Schools, Mr. J. W. MCCLYMONDS, who is also the Director of the Observatory.

Until the Observatory was rebuilt the attendance gradually increased each year, from 1644 in 1886-7 to 2240 in 1891-2, but since November, 1892, when the new building was opened, it has been impossible to keep anything like a correct register. For many nights when the lecture room alone is used no account is kept, but it is well within the limits to say there are fully 5000 visitors a year.

To determine the effect of the electric street lights, experiments were made in stellar photography during the past winter and very encouraging results obtained. At my request, Mr. JOHN A. BRITTON, the Secretary of the Electric Light Co., altered all the lamps visible from the Observatory, so that the cone of shadow thrown by the arms of the lamps fell upon the equatorial dome, and the change for the better was very great. One lamp only about 300 feet from the Observatory, arranged as above, has no more evil effect visually, than a naked gaslight at the same distance. The BAUSCH & LOMB OPTICAL CO. of Rochester, N. Y., are at work upon a 4-inch (doublet) lens to carry on this photographic work during the coming winter.

CHABOT OBSERVATORY, March, 1894.

THE HARVEST MOON.

BY ORRIN E. HARMON.

In September, 1894, for the first time in nearly nineteen years, the harvest Moon will appear in her full splendor. As the Moon's ascending node will be only $1^{\circ} 35'$ from the vernal equinox on the 14th of September, the date of full Moon, the inclination of her orbit to the horizon will be very nearly the same as if the node were exactly at the equinox.

As every student of astronomy knows, the effect of this position of the Moon's ascending node is to render the inclination of her orbit to the horizon a minimum when the vernal equinox is in the eastern horizon. Hence, the intervals between the times of the Moon's rising, near the September full Moon, are also at a minimum. For the purpose of showing these intervals I have prepared the following table, giving the times of the Moon's rising from September 13 to September 21, inclusive. This table is computed for the parallels of Lat. 37° and 47° , and for the meridian of 8 hours :

1894.	LATITUDE 37° N.		LATITUDE 47° N.	
	MOON RISES.	INTERVAL.	MOON RISES.	INTERVAL.
	H. M.	H. M.	H. M.	H. M.
September 13 . . .	5 41	. . .	5 54	. . .
" 14 . . .	6 06	0 25	6 10	0 16
" 15 . . .	6 27	0 21	6 25	0 15
" 16 . . .	6 52	0 25	6 39	0 14
" 17 . . .	7 14	0 22	6 57	0 18
" 18 . . .	7 44	0 30	7 17	0 20
" 19 . . .	8 21	0 37	7 44	0 27
" 20 . . .	9 05	0 44	8 20	0 36
" 21 . . .	9 58	0 53	9 09	0 49

Remembering that the average interval between the times of the Moon's rising is about 51^m , the intervals above noted, for the few evenings after the full Moon, show a marked diminution from the mean interval.

Now the very cause that makes the inclination of the Moon's orbit to the horizon a minimum at the September full Moon, makes this inclination a maximum at the March full Moon. Full Moon happens March 21, 1894, and the ascending node will then

be about 11° from the vernal equinox. Of course, the inclination of the Moon's path to the horizon is not quite so large as it would be if the ascending node were at the equinox; but it is sufficiently large to show a decided increase in the intervals between the times of the Moon's rising. Between March 20 and March 26, at Lat. 37° north, these intervals vary between $1^{\text{h}} 02^{\text{m}}$ and $1^{\text{h}} 09^{\text{m}}$; and at Lat. 47° north, between $1^{\text{h}} 09^{\text{m}}$ and $1^{\text{h}} 16^{\text{m}}$.

As we watch the Moon's rising during the evenings of the harvest Moon, it is interesting to note the changing positions of her path with respect to the horizon and meridian. To show this change I have made a calculation, and the results are given in the following table. I have selected the time of the Moon's rising on September 13 and September 21:

1894. ELEMENTS OF THE MOON'S PATH.	LATITUDE 37° N.		LATITUDE 47° N.	
	SEPT. 13. 5 ^h 41 ^m	SEPT. 21. 9 ^h 58 ^m	SEPT. 13. 5 ^h 54 ^m	SEPT. 21. 9 ^h 9 ^m
	° /	° /	° /	° /
Declination of Meridian crossing-point	— 28 12	— 15 04	— 28 27	— 20 02
Angle of Meridian cross- ing-point	84 23	65 16	86 00	68 59
Inclination to Horizon . .	25 23	44 15	15 04	30 45
Amplitude in Horizon . .	13 11	36 51	15 33	44 33
	S. of E. and N. of W.	N. of E. and S. of W.	S. of E. and N. of W.	N. of E. and S. of W.

On the 14th of October, 1894, the Moon's ascending node will be exactly at the vernal equinox. Eighteen years and seven months will elapse before the node will again be at the equinox. The westward motion of the Moon's nodes on the ecliptic is one of the many beautiful movements which the mechanism of the heavens displays. But while her nodes are making this circuit on the ecliptic, what is the position of her equatorial nodes or the lunar equinoxes? A simple calculation shows that they vibrate each side of the solar equinoxes, always keeping within the limit of $13^{\circ} 9'$. There is a remarkable agreement between these figures and the figures expressing the Moon's mean daily motion among the stars.

CHEHALIS, Lewis Co., Washington.

CAN ORGANIC LIFE EXIST IN THE PLANETARY SYSTEM OUTSIDE OF THE EARTH?

BY C. A. STETEFELDT.

In the following discussion I hold to the principle, expressed in NEWTON's *Regulæ Philosophandi*, that in explaining any phenomena taking place outside of our Earth, we must not assume the existence of new and unknown forces and properties of matter. On the contrary, the more our knowledge of the universe progresses, the more we become convinced that the phenomena observed on the Earth only repeat themselves on a smaller or grander scale in every nook of infinite space.

Philosophers have told us that the universe has no reality, but exists only in our imagination. Gravity would still exist if there were no arms to lift; sound-waves would exist if there were no ears to hear; light and heat-waves would exist if there were no eyes to see and no nerves to feel. These philosophers should have said: The imperfections of our sensory organs fail to bring to our consciousness thousands of modes of energy that have a real existence. On the whole, speculative philosophy, not based on the real, has done almost nothing to widen the horizon of our knowledge of the universe.

Scientists, however, should not throw stones at these philosophers when many of the former have themselves lived in glass houses. I refer here to certain strange speculations advanced, in violation of all known laws of physics and biology, by some astronomers in regard to the existence of organic life and rational beings on the stars, the Sun, and the planets and their satellites.

So far as the habitability of the Sun and the fixed stars is concerned, we might dismiss this at once. I quote, however, the strange hypothesis of the Sun's photosphere, brought forward by Sir WILLIAM HERSCHEL,* which assumed that the nucleus of the Sun was actually cold,—cold enough to sustain organic life—and that the heat and light we receive was generated in a mysterious way in the Sun's atmosphere, high above its solid body. How the Sun's inhabitants escaped destruction, the hypothesis did not sufficiently explain. It is also stated that the Sun-spots are

* With the theory of heat current in HERSCHEL'S time, his conclusions were not by any means so absurd as they would be to-day.

holes or windows in the upper photosphere, made by the Creator for the purpose of enabling the solar astronomers to get a glimpse of the universe of which they would remain in entire ignorance otherwise.

We must, however, acknowledge that if other Suns in the universe have planets—and there is no reason why they should not—many of them may present physical conditions identical with or similar to those existing on Earth, and that therefore their organic life may be similar to our own. Further, I am far from denying that under favorable circumstances creatures may be evolved upon planets which revolve around other suns, whose mental capacity is as much superior to man's as that of the latter is to the lowest forms of our vertebrates.

I remark here that our imagination is strictly confined to reproducing the images of terrestrial forms and phenomena, the impressions of which our senses have left upon the brain. The human form has always been, and is now, the artist's ideal of beauty, and beyond he cannot go. Wherever an attempt is made to represent anything super-human, attributes are derived from animals lower than man. Thus, we see horns on the head of MICHAEL ANGELO'S Moses; angels appear with the conventional wings of birds; the devil has a tail and a cloven foot. MICHAEL ANGELO represents in his frêscò "The Creation of Eve" the god of Christianity as an old man with a long, white beard; the holy ghost always appears as a dove; heaven and hell, as depicted by DANTE and MILTON, could be easily set in scene on the stage of a theatre. The gods described by the poets of the ancients were only men and women in disguise; and even God, as described by some Christian theologians, is a being with purely human attributes, sitting on a throne in heaven, surrounded by an army of winged angels.

If we acknowledge the uniformity of matter in the universe, and spectrum analysis leaves us no other alternative; if we acknowledge that chemism is everywhere the same, at least at that stage in the development of a planetary system when the celestial nebulæ have commenced to condense,—and we have no reason to assume otherwise:—then the physical conditions for the existence of organic life are exceedingly limited, *i. e.*, they depend on certain temperatures and atmospheric densities and constitution. Organisms, while they can live through spells of temperature much below the freezing-point of water, do not thrive at such

temperatures; they are killed at the boiling-point of water at normal atmospheric pressure, and all so-called organic compounds decompose completely at higher temperatures.

Paleontology teaches us that a continuous progress took place in the evolution of organic life caused by comparatively slight changes in temperature and atmospheric conditions.

At the time when the first protoplasm was formed we may assume that the Earth was not as cold as it is now, that its crust had not yet attained sufficient thickness to prevent the conduction of internal heat to the surface; and judging from the enormous deposits of coal and mineral oil in its strata, the atmosphere may have contained originally a much larger percentage of carbonic acid than at present, thus favoring a more luxurious growth of plants. The latter facts, the higher temperature, and in consequence, the greater quantity of water in the atmosphere, by which more intense atmospheric disturbances were produced, prevented at that time the evolution of organisms of the highest type. In examining the distribution of organic life on the Earth, it is easily seen how narrow are the limits for the existence of the highest types. Compare the flora and fauna of the equatorial and polar zones, and note that man, the noblest of beasts, finds his highest mental development only in the temperate zone.

But there are other points to consider: I contend that in order to sustain high types of life on a planet or satellite a certain density of its atmosphere is necessary; that its period of rotation round its axis should not be too long; that the planet should not be too far distant from the Sun.

Concerning the first proposition, it is evident that if the atmospheric density is almost imperceptible, life cannot be sustained at all, and that an enormous density would prevent at least the existence of higher organisms, even if they easily accommodated themselves to great pressure, on account of the destructive influence of atmospheric phenomena. Consider the effect of a Kansas cyclone, and imagine the result if our atmosphere had a density a thousand times greater.

Concerning the second proposition, it is clear that if the time of rotation of a planet round its axis is unreasonably long, as, for instance, with the Moon, the difference in temperature between day and night, other conditions being equal, would be so great that high types of life could not be evolved. Paleontology and the distribution of living organisms on our globe point to the fact

that evolution was most intense, and reached its highest types, where favorable atmospheric conditions were most uniform. Our ancestors, the apes, were not born on the north pole.

Finally, if planets are so far removed from the Sun that the light and the heat they receive is only a small fraction of that supplied to our Earth, the conditions for sustaining high types of organic life cannot be favorable either. The annexed table, with figures referring to the most important planets, explains itself. It is not necessary to consider satellites outside of the Moon, or the army of minor planets with diameters of not much above 200 miles down to a few miles. The figures quoted are taken from *Young's Astronomy*, edition 1888, with exception of the atmospheric densities.

Regarding the atmospheres of planets, astronomers have made some strange statements. The only rational treatment of this subject I found in ZOELLNER's *Die Natur der Cometen*, and the atmospheric densities given in the table are calculated from ZOELLNER's equation. According to well-known physical laws, the density of so-called permanent gases on the surface of a planet, or any celestial body, must be a function of gravitation on its surface, and of its radius. But I go further and contend:

- (1) That all planets of the solar system must have atmospheres of permanent gases similar in composition to our own atmosphere.
- (2) That it is untenable to speak of the limitation of an atmosphere; that, on the contrary, the whole interplanetary and interstellar space must be filled with these exceedingly attenuated gases.
- (3) That if a new planet were put in this space, it must collect and abstract from the atmospheres of other bodies an atmosphere corresponding to its own mass and radius.

Besides, there are gases and vapors in planetary atmospheres which distribute themselves according to DALTON's law, and which follow the laws of MARIOTTE and GAY LUSSAC only within certain limits. I refer here, principally, to the existence of water in planetary atmospheres. Various observations leave no doubt that water, in one or more of its states of aggregation, is present on all the planets, and especially on the smaller ones.

I call attention to the low specific gravities of the Sun and the four large planets. Let us first consider the Sun. The disk of the Sun, as it appears to the naked eye and through the telescope, does not represent the actual surface of the Sun's liquid or partly solid nucleus, but the latter plus the photosphere, *i. e.*, the

envelope of highly incandescent gases, vapors, and floating, condensed particles. Beyond the photosphere is an extensive atmosphere of permanent gases, principally hydrogen, called the chromosphere. We cannot ascertain the height of the photosphere, and consequently can have no knowledge of the actual diameter and specific gravity of the Sun's nucleus.

A similar argument holds good for the large planets *Jupiter*, *Saturn*, *Uranus*, and *Neptune*. There seems to be no doubt that these planets have not yet cooled down as much as the Earth and the other smaller planets; that, on the contrary, they still radiate a considerable amount of their own heat. This would necessarily lead to the formation of very dense, cloudy envelopes, and to the enlargement of their visible disks far beyond the diameters of their solid bodies.

ZOELLNER'S equation, from which the atmospheric densities have been calculated, is as follows :

$$\text{Log. nat. } \frac{d}{d_i} = \frac{1}{a. a.} \left(\frac{g. r.}{t} - \frac{g_i r_i}{t_i} \right)$$

d Density of air on surface of planet.

g Gravitation “ “ “

r Radius of planet.

t Absolute temperature of the space in which the planet exists.

d_i, *g_i*, *r_i*, *t_i* The same values for the Earth.

$\frac{1}{a}$ Absolute temperature of melting ice.

a Constant for air.

The following values are assumed :

$$d_i = 1;$$

$$g_i = 0.00981 \text{ kilometres};$$

$$r_i = 6367.4 \text{ kilometres};$$

$$\frac{1}{a} = 273;$$

$$a = 0.07838.$$

For the sake of simplicity we put

$$t = t_i = 273^{\circ} \text{ C.}$$

This gives the equation :

$$\text{Log. nat. } d = 12.76 (g r - 62.46).$$

The atmospheric densities thus obtained are not entirely correct, but close enough to the truth for our purposes. It is evident that in putting $t > t_1$, for *Venus* and *Mercury*, the equation would give still higher negative values, *i. e.*, still lower densities; and putting $t < t_1$, for the four large planets, their atmospheric densities would be still greater. The same notation holds good if we assume a lower absolute temperature for interplanetary space. In all cases we obtain almost imperceptible densities for the Moon and the small planets, and enormous figures for the large planets. Outside of this, a correction would have to be made dependent on the temperature of the planet's surface which, however, would not change results materially either, *i. e.*, so far as the question at issue is concerned.

The enormous atmospheric densities of the large planets, as deduced from ZOELLNER's equation, lead us to another interesting consideration. It is hardly necessary to state that these figures can have no real existence, or the apparent specific gravities of the Sun and the large planets would be much greater. They result in consequence of a deficiency in ZOELLNER's equation which assumes that MARIOTTE's law has no limitation. Although there exists, according to ANDREWS, for each gas a critical temperature above which no amount of pressure can convert it into a liquid, the law of MARIOTTE, or a similar law, cannot even then hold good for unlimited pressure. Whenever a certain limit has been reached, the gas acts like a liquid, *i. e.*, increased pressure fails to reduce its volume. According to the atomic theory of matter and ether, the atoms are rigid and of distinct shape. When the ether-spheres have been compressed to the last limit, and the molecules of matter have been brought to the least possible distance, no amount of pressure can produce a further reduction of volume. I am not aware that experiments have been made to settle this question for any gas at a temperature above the critical point. These defects in ZOELLNER's equation, however, do not influence results as a whole, *i. e.*, the fact remains that the smaller planets must have almost imperceptible atmospheres, and the large planets enormously dense ones.

Finally, we have to take into consideration that the temperature on the surface of a planet depends not only on its distance from the Sun, *i. e.*, on the quantity of heat received, but also on the density of its atmosphere. If the latter is exceedingly slight, the heat cannot be retained but is radiated back into space as fast

as received. In proof of this, I only need to remind the reader that even in the equatorial zone high mountains are covered with eternal snow.

On planets with slight atmospheres of permanent gases a lively evaporation of water, if such be present, must take place, and these atmospheres must contain more water-vapor than air. This, as will be seen later on, is actually the case.

THE MOON.

A glance at the figures in the following table is sufficient to show that the lunar citizen and his flower garden are a chimera. Hence, we might dismiss this matter without further argument; but it is of interest to obtain more proof by actual observation. The telescopic appearance of the Moon gives no evidence of atmospheric phenomena. There is absolutely no refraction when the Moon passes between us and a star. The lunar spectrum is exactly like the Sun's, being only reflected sunlight. The heat we receive from the Moon is so slight that exact measurements do not exist. A large portion of it is reflected sun-heat; the rest is obscure heat, *i. e.*, heat which has been first absorbed by the Moon's surface and then radiated. Again, a portion of the latter in the lunar heat-spectrum consists, according to LANGLEY'S measurements with the bolometer, of heat-waves having a greater length than waves radiated from a block of ice. Hence, the temperature on the Moon's surface must be very low. But why is the temperature on the Moon so low when a lunar day is so long, the Sun shining uninterruptedly for fourteen days on the same spot? The explanation is exceedingly simple. In the first place, the lunar atmosphere being practically non-existent as compared with our own, heat cannot be retained on the Moon's surface. Then it is held by some astronomers that a considerable portion of the Moon's surface is covered by ice and snow. By evaporation of the ice the Sun's heat is absorbed, and the water-vapor travels to the Moon's side averted from the Sun, where it again condenses. But if so, would not this vapor show atmospheric phenomena, *e. g.*, refraction of light? This does not follow; according to ZOELLNER'S calculations even this atmosphere of water-vapor is so attenuated that it would escape observation.

Astronomers, however, were grieved to rob Luna of her man, and HANSEN gallantly embraced her cause. He calculated that the center of the Moon's gravity is some thirty miles farther from us

than the center of its figure, *i. e.*, the side of the Moon we see is bulged out. Thus, a table-land of enormous height exists on this side of the Moon, with an imperceptible atmosphere, while the other side, averted from us, is provided with an ample atmosphere, breathed by happy lunar citizens. They have, however, one great sorrow: They can never get a glimpse of the Earth, because it means death to them to take a trip to the highlands. HANSEN'S conclusions have been shown to be unwarranted by facts; but assuming that they were correct, the Moon's atmosphere would not be perceptible even on the depressed side.

MERCURY.

Outside of an almost imperceptible atmosphere of permanent gases, which would exclude organic life, *Mercury* presents another ugly feature. The eccentricity of his orbit is so large that he receives $2\frac{1}{4}$ times more heat near perihelion than near aphelion. Being so near the Sun, he receives, in the mean, $7\frac{1}{2}$ times as much light, and heat as the Earth. All this must cause the most violent climatic changes under which no highly organized forms of life could very well exist. That *Mercury* has, however, a perceptible atmosphere of water-vapor is proved by lines in his spectrum and by phenomena observed at transits, and this is not surprising in consideration of the large amount of heat received from the Sun, by which a lively evaporation of water must be effected.

VENUS.

Venus shows neither very conspicuous surface-markings nor atmospheric phenomena. The spectroscope proves the presence of water in her atmosphere, and the latter becomes especially perceptible in transits, *i. e.*, when *Venus* passes between the Earth and the Sun. Unfortunately, these transits are rare. The last one took place in 1882, and the next one will occur in 2004. I quote from *Young's Astronomy*: "When the planet is near the Sun, the horns of the crescent extend notably beyond the diameter, and when very near the Sun, a thin line of light has been seen by several observers to complete the whole circumference. This is due to refraction of sunlight by the planet's atmosphere, a phenomenon still better seen as the planet is entering upon the Sun's disk at a transit, when the black disk is surrounded by a beautiful ring of light."

I beg to disagree, however, with the following conclusion,

based on the above observations: "Its atmosphere is probably from one and a half to two times as extensive and dense as our own."

Photometric researches, namely, the high albedo and the peculiar distribution of light in the phases of *Venus*, especially the great light-intensity of small phases, make it highly probable that this planet is largely covered by water. And considering that *Venus* receives nearly twice as much sunlight and heat as the Earth, evaporation of water must be very intense in its rarified atmosphere of permanent gases. This atmosphere of water-vapor would fully account for the phenomena observed without assuming the correctness of the statement regarding atmospheric density and extent.

In fact, observations made by different persons, at the same time, and at different transits, do not agree at all, and leave it doubtful how much of the phenomenon is reality and how much optical illusion.

In Mercurial transits similar phenomena, but of less intensity, are observed.

MARS.

Mars shows evidence of polar ice-caps changeable with the seasons. The latter must be more pronounced than with us, the inclination of equator to orbit being slightly, and the eccentricity of orbit very much, greater. There are other distinct markings on this planet which have the appearance of land and sea. The spectroscope proves the presence of water in its atmosphere. Its attenuated atmosphere of permanent gases, which it shares with the Moon, *Mercury* and *Venus*, would positively exclude life in this as well as in all other cases.

JUPITER, SATURN, URANUS AND NEPTUNE.

All observations, into which I need not enter here in detail, prove that *Jupiter* and *Saturn* have not yet cooled down like the smaller planets; but still retain considerable heat and are slightly self-luminous. This is not surprising if we consider their enormous masses, and that according to investigations of BUFFON, BISHOP and others, the time required for cooling a hot body increases very rapidly with an increase of its mass. The atmospheric disturbances of these planets are so violent that they could not be produced by the Sun's heat alone which, as we find in the table, is only about $\frac{1}{16}$ and $\frac{1}{16}$ respectively of that received by

the Earth. This would at once negative the existence of organic life. The same argument holds good for *Uranus* and *Neptune*, but in a less degree. They are so far removed from us that observation of their atmospheric phenomena becomes very difficult. While they must be much cooler than *Jupiter* and *Saturn*, and may not be self-luminous, they evidently still retain a high temperature as indicated by their spectra. Assuming, however, that *Neptune*, for instance, had cooled down completely, he would receive only about $\frac{1}{10000}$ of the Sun's light and heat we get. So far as light is concerned, this would not be so bad ; it would still be equal to a one thousand candle-power electric lamp at a distance of $10\frac{1}{2}$ feet. By far the larger portion of the light, however, would be absorbed by the dense, enormous atmosphere of the planet, and it is questionable whether the disk of the Sun would ever be visible on its surface, and a direct ray of sunlight be able to penetrate the gloom. On account of the great atmospheric density, sufficient heat from the Sun may be retained on the surface of the planet to support organic life. But even then, *i. e.*, after cooling down sufficiently, this planet and the other large ones could only evolve certain low forms of living creatures on account of the enormous atmospheric densities and corresponding atmospheric disturbances.

In concluding this investigation we cannot help admiring the inductive acumen of the theologians who considered the Earth the most important of the planets, and the center of creation. Although their opinions were not based upon scientific facts, they arrived at the truth, nevertheless.

	Mass. Earth = 1.	Mean Diameter. Earth = 1.	SPECIFIC GRAVITY.		Mean Gravity at Surface. Earth = 1.	Product of Gravity and Radius in Kilom.	Atmospheric Density at Surface. Earth = 1.	Light and Heat Received. Earth = 1.	Time of Axial Rotation.	Eccentricity of Orbit.	Inclination of Equator to Orbit.
			Earth. = 1.	Water. = 1.							
SUN	331100	109.4	0.25	1.39	27.65	. . .	. . .	. . .	. . .	. . .	. . .
MERCURY.	0.066	0.380	1.26	7.14	0.47	11.15	$\frac{1}{10^{284}}$	6.7	Practically the same	0.2056	?
VENUS.	0.78	0.972	0.86	4.85	0.83	50.38	$\frac{1}{10^{67}}$	1.93	for the four small planets.	0.0068	?
EARTH.	1.00	1.00	1.00	5.58	1.00	62.46	1	1.00		0.0167	23° 27'
MOON	0.0123	0.273	0.61	3.40	0.166	2.83	$\frac{1}{10^{348}}$	1.00	27 ^d 7 ^h	. . .	. . .
MARS	0.107	0.534	0.72	4.01	0.38	12.67	$\frac{1}{10^{275}}$	0.44	. . .	0.0932	24° 50'
JUPITER	316	10.92	0.24	1.33	2.65	1807.47	10^{870}	0.0372	About 10 ^h for these two.	0.0485	3° 05'
SATURN	94	8.87	0.13	0.72	1.18	661.07	10^{323}	0.0111		0.0560	26° 49'
URANUS	14.7	4.03	0.22	1.22	0.91	229.04	10^{223}	0.0026	?	0.0463	?
NEPTUNE.	17.1	4.39	0.20	1.11	0.88	241.28	10^{660}	0.0011	?	0.0089	?

THE NEW STAR IN *NORMA*.

BY W. W. CAMPBELL.

Attention was called in these *Publications* (Vol. V., page 225, Vol. VI., pages 52-54), to the discovery of a new star of the 7th magnitude in the constellation *Norma*. It was found by means of its peculiar spectrum recorded on a photograph of that part of the sky, made July 10, 1893, by the HARVARD College observers at Arequipa, Peru. The actual discovery was not made till late in October, when the plate was examined by Mrs. FLEMING at Cambridge, Mass. By that time the Sun was near the region of the *Nova*, and, probably for that reason, no other spectrum photographs were obtained. But the original negative sufficed to establish the extremely interesting fact that its spectrum was essentially identical with the spectrum of *Nova Aurigæ* in February, 1892.

The Declination of the new star is south $50^{\circ} 14'$, and the latitude of Mount Hamilton is north $37^{\circ} 20'$; so that the true altitude of the star when on our meridian is only $2^{\circ} 26'$. Any observations secured here could only be successful to a very limited degree.

The *Nova* first came into accessible position in February. It was found, without difficulty, the morning of February 14. Its light was estimated at one-fifth or one-sixth that of the adjacent 8th magnitude star, Cordoba *General Catalogue*, 20940. The *Nova* would therefore be of the $9\frac{1}{2}$ or 10th magnitude.

Its spectrum consisted of an exceedingly faint continuous part in the yellow and green, and four bright lines apparently identical in position and relative intensity with the bright lines at wave-lengths 501, 496, 486, 575 in the August, 1892, spectrum of *Nova Aurigæ*. Rough measures of the wave-lengths of the brightest two lines, made after daylight, gave 5013 and 4953. While the measures were not satisfactorily made, there was no doubt that the spectrum was that of a planetary nebula. The star has been seen on four subsequent mornings. Its magnitude has undergone no appreciable change. Fairly satisfactory measures of the brightest three lines give as their wave-lengths, 5007.3, 4957, 4860. The latter is the hydrogen H β line, whose normal position is at 4861.5. The normal positions of the first two lines in the well-known nebulae, according to Dr. KEELER'S,

1890 observations, are 5007.0 and 4959. The line in the yellow is too faint for measurement. The lines are broad, as in the spectrum of *Nova Aurigæ*. More accurate measures than the above cannot be made here, and subsequent observations will be confined to noting any possible variations in brightness.

Both *Nova Aurigæ* and *Nova Normæ* at discovery possessed substantially identical spectra of bright and dark lines. Both diminished in brightness and both assumed the nebular type of spectrum. The new star of 1876 in the constellation *Cygnus* probably had an identical history: passing from a bright star with a spectrum of bright and dark lines, to a faint star with a spectrum in which only one bright line was seen (probably the bright nebular line at 5007). We cannot say just what the significance of two or three "new stars" with identical histories will be, but it will be great. Only five new stars have been discovered since the application of the spectroscope to astronomical investigation, and that three of these should have similar spectroscopic histories is a remarkable fact, and an encouraging fact to students of these most interesting objects.

AN EXPLANATION OF THE BRIGHT PROJECTIONS OBSERVED ON THE TERMINATOR OF *MARS*.

BY W. W. CAMPBELL.

An extremely favorable opposition of *Mars* is rapidly approaching. Useful observations can probably begin in June and continue into the year 1895. It is known that many astronomers are preparing to expend a great deal of energy upon that planet, and are expecting more valuable results than have been obtained at any previous opposition. We shall undoubtedly have interesting accounts of the progress of their work, both in the scientific journals and in the public press, which everyone, astronomers included, will welcome. It is, unfortunately, perfectly safe to predict that we shall also hear from the sensationalists, astronomers included; and that fact is a source of sincere regret to all healthy minds.

Nearly all the problems concerning *Mars* are still awaiting solution. It is difficult to mention any, indeed, except some of those relating to the polar caps, that have been solved

satisfactorily. There are a few points to which I wish to refer before the observing season begins, which, it seems to me, are of great significance.

SCHIAPARELLI has often called attention to the appearance of intensely bright spots upon the planet, and he has attributed to them very great importance in the study of its physical constitution. While this distinguished observer has not, so far as I know, seen these bright regions *projecting beyond the terminator*, he has made it plain* that they are relatively much brighter at the edge of the planet than near its center. That they are phenomena connected with the surface of the planet, in some way, follows from their more or less permanent character.

Three white spots were observed on eight or ten nights in 1888 by TERBY† which were invisible until they approached the western edge of the disc, when they became very bright, and before passing behind the planet projected beyond the edge of the disc by irradiation, as is the case with the polar cap. While it is not clear to me that all of M. TERBY's observations refer to the same three points, it is probable that most of them do, and that the points were more or less permanent in their character. The terminator was on the western edge of the disc during the first few nights of the observations referred to, but for the greater number the terminator was on the eastern edge.

White spots, similar to those described by SCHIAPARELLI and TERBY, were frequently observed at Mount Hamilton in 1888, 1890 and 1892; but the brilliant projections on the terminator, first observed here in 1890, are of a much more extreme character; and aside from the polar caps are possibly the most interesting and significant phenomena ever observed on *Mars*. The observations of 1890 are described in *Publications A. S. P.*, Vol. II, pages 248-9, from which we quote:

"The interesting phenomenon of bright spots projecting beyond the terminator of *Mars*, and presenting much the same appearance as the summits of lunar mountains and craters when first visible outside the terminator of the Moon, was well seen with the 36-inch refractor on the nights of July 5 and 6. The attention of the astronomers was directed to the aspect of *Mars* on July 5 at 10^h Pacific Standard time, by a visitor, who happened

* SCHIAPARELLI's account of the bright spots observed by him is reprinted on later pages of this publication.

† Described in *Mémoires Couronnés, Académie Royale de Belgique*, Vol. LI.

to be looking in the telescope at that time, on one of the public nights of the Observatory. A sketch made by J. E. K. at this time shows a narrow, elliptical, white spot, from $1''.5$ to $2''.0$ long, projecting downward (northward) at a small angle with the line of the terminator. The seeing was 5, or the best which is known at the Observatory. At $10^h 30^m$ the spot was within the disc, but still visible as an oval white patch on a darker background.

"On July 6 the same appearance was more carefully observed by E. S. H., J. M. S. and J. E. K. A projecting spot was seen by E. S. H. at $8^h 3^m$ Pacific Standard time. At $8^h 45^m$ it curved upward and nearly met another smaller projecting spot some $2''$ farther toward the south. J. M. S. considered that there was an actual connection, although it was very faint.

"The lower spot, although it changed considerably in shape, remained visible for more than an hour, and was observed to be always situated on the end of a long bright stripe of the surface of the planet which lies north of *Deuteronilus*. The simplest interpretation of the phenomenon is therefore that this strip is (or was at the time of observation) elevated above the general surface. At about $10^h 25^m$ of July 6 the aspect was much the same as that of the spot seen on the preceding night and was no doubt produced by the same part of the planet."

Careful sketches of the appearances were made.

It should be stated that *Mars* was not observed at all on the nights preceding and following July 5 and 6, as the telescope was used in other work, and it cannot be said on how many nights these projections continued.

The projections were real. They were not caused by irradiation. They were not only elevated above the theoretical terminator, but the prominent ones curved round to the north into a position parallel with the terminator, and the northern end of the curved arm was separated from the illuminated disc by a dark line of appreciable breadth.

The latitude of the principal projection was very nearly $+40^\circ$. I have computed the longitudes of the terminator at $+40^\circ$ and found them to be as follows :

July 5 ^d	10 ^h	0 ^m	Longitude = $44^\circ.8$
	10	30	52 .1
6	8	3	6 .3
	8	45	18 .9
	10	25	40 .7

A comparison with SCHIAPARELLI'S map will show that the projection observed July 5 covered the preceding end of the very bright region *Tempe*. The terminator at 10^h 25^m on July 6 was just at the beginning of *Tempe* and the bright projection, which was rapidly assuming its form as observed at 10^h of the preceding night, was covering the preceding end of *Tempe*.

Similar projections were observed on the terminator by Messrs. HOLDEN, SCHAEBERLE, HUSSEY and CAMPBELL at the opposition of 1892. Concerning these observations Professor HOLDEN says, in *Astronomy and Astro-Physics* for October, 1892 :

"A very interesting series of observations of bright projecting points on the terminator of the planet was begun by Mr. SCHAEBERLE and myself early in June, and was continued till the middle of July. * * * During the opposition of 1890, similar observations seemed to show that these projections were the prolongation of white streaks on the planet (clouds?), and the phenomena of 1892 have been examined to see if a similar conclusion would hold. The results have, so far, been indecisive."

Professor SCHAEBERLE has written as follows concerning his observations of the projections in June and July, 1892 :

"Where these dark streaks seem to intersect the limb of *Mars* the white areas projecting beyond the terminator of *Mars* (often noticed in June and July during the partial phase) were usually seen, indicating that the dark streaks were elevated above the general surface and rendered more bright by being projected against a dark background. That the bright areas projecting beyond the terminator actually lie in a dark streak needs further confirmation."—*Publications A. S. P.*, Vol. III, page 197.

M. PERROTIN, Director of the Nice Observatory, with their 30-inch equatorial, observed projections on the terminator of *Mars* on three occasions in 1892. His account of the observations was presented to the French Academy, and published in *Comptes Rendus*, Vol. CXV, pages 379-381. Concerning them he says :

"I would perhaps have still further delayed the sending of my letter if I had not received in the last few days from M. NEWCOMB an extract from a journal in which it is reported that the LICK astronomers have likewise observed the luminous projections on the edge of the disc."

The first projection was seen by M. PERROTIN on June 10,

from $15^h 12^m$ to $16^h 17^m$ Nice mean time, in the vicinity of -30° latitude. On July 2 a projection was visible at $14^h 10^m$ when observations for the night began, and disappeared at about $14^h 40^m$. Again, on July 3, the projection was visible at $14^h 11^m$ to $15^h 6^m$. Those of July 2 and 3 were estimated to be in latitude -50° , and to be produced in the same region of the planet. M. PERROTIN's comments are as follows :

"Such are the facts. I shall not venture to interpret them. They were seen with such great distinctness that it is scarcely possible to consider them as the result of some illusion.

"Moreover, as we are here considering projections outside the disc of at least one or two tenths of a second of arc—that is to say, phenomena occurring at altitudes greater than 30 or 60 kilometres—the mind finds itself overwhelmed with such numbers, to which we are not accustomed on our globe, and it is doubtless only to phenomena purely luminous that we can look for the explanation of such altitudes."

Assuming the latitudes to be -30° , -50° and -50° on the three dates, I have computed the longitudes of the terminator at the points occupied by the projections observed by M. PERROTIN with the results :

1892 June	$10^d 15^h 12^m$ to $16^h 17^m$	$\beta = -30^\circ$	$\lambda = 212^\circ.4$ to $228^\circ.2$
July	2 14 10 to 14 40	-50°	335 .4 to 342 .8
	3 14 11 to 15 6	-50°	325 .7 to 339 .0

Professor HUSSEY, who took part in the LICK observations of *Mars* during July and August, 1892, observed one or more projections on every good night that he worked up to and including July 17, viz.: July 2, 5, 10, 11, 12, 13, 17. My observations were limited to parts of four nights a week between July 10 and August 17. I observed these projections on July 10, 11, 12, 13, 17; that is, on the first five nights that I used the telescope. I believe that no one here saw the projections *after* July 17, and that all the projections lay between latitudes -30° and -50° , with the exception of one or two minor prominences observed at about -25° . Those observed by Mr. HUSSEY and by me were all included between longitudes 310° and 95° . The most prominent ones were those observed on July 11 and 13. On July 11 two prominences were visible. The most striking one was visible when the observations began at $12^h 15^m$, and remained constantly in view for about two hours. Its shape changed a great deal during that time. At $13^h 25^m$ it was unusually prominent, and its

outer extremity was perceptibly bent upward toward the south polar cap. Measures of the position of the projection gave it an average latitude of -47° , but its south latitude continually increased. My notes on this point are, " $13^{\text{h}} 25^{\text{m}}$, distance from prominence to cap is less than at $12^{\text{h}} 55^{\text{m}}$;" and " $13^{\text{h}} 55^{\text{m}}$ prominence smaller and closer to cap than before." The longitude of the prominence varied from 340° to 7° . I did not reduce these observations till January, 1894, when a comparison of the results with SCHIAPARELLI's maps showed pretty conclusively that the projections were either central on the bright region *Noachis* or on its southern border. The following end of *Noachis* approaches the polar cap, just as the projections so plainly did. A second and less prominent projection in latitude -33° was visible from $12^{\text{h}} 45^{\text{m}}$ to $13^{\text{h}} 55^{\text{m}}$ the same night.

On July 13, the observations of July 11 were substantially duplicated. The southern one of the two projections presented the hooked or bent appearance most strongly at $14^{\text{h}} 35^{\text{m}}$, just as it was seen two nights earlier at $13^{\text{h}} 25^{\text{m}}$; and therefore, allowing for the longer rotation period of *Mars*, the same point on the planet was under observation on the two nights. The position of the base of the prominence was latitude -46° , longitude 357° . The same part of the planet was on the terminator when observations were being made on July 10 and 12, and projections were seen at those times also, though the seeing was not so good.

It is not intended to include the details of the LICK observations of these projections in this paper. They will be published later in the general report of all the *Mars* observations made here in 1892 and 1894.

The only reference to these bright projections by the Arequipa observers appears to be the following statement by Mr. W. H. PICKERING in the December, 1892, *Astronomy and Astro-Physics*: "Clouds have on several occasions been observed to project beyond the terminator and also beyond the limb, thus confirming the observations made at the LICK Observatory. The height of some of these clouds has been measured, and it appears that some of them attained an altitude of at least twenty miles—a height considerably greater than that attained by terrestrial clouds. This is a result naturally to be expected from the small mass of the planet."

Such are the observations of the bright projections, and the

comments made by the observers. They do not seem to have received further attention from astronomers interested in Martian problems. Miss CLERKE adopts for her valuable *History of Astronomy* the observations and interpretations made by Mr. W. H. PICKERING; she attributes them likewise to the LICK observers and to M. PERROTIN, which is hardly correct. I believe the LICK astronomers and M. PERROTIN, so far as published accounts go, saw the projections only beyond the *terminator*, indeed on a very limited part of the terminator, and not at all beyond the *limb*.

I believe these projections can be explained on the hypothesis that they are due to mountain chains lying *across* the terminator of the planet, possibly covered with snow in some cases, and in others not necessarily so.

It is reasonable to suppose that mountains exist on *Mars*, and that they ought to be visible in large telescopes. The distance of the planet from the Earth on July 11, 1892, was approximately 39,000,000 miles. We were able to use powers of 350 to 520 diameters in our observations. The equivalent distances of the planet under those powers were 110,000 miles and 75,000 miles. Our Moon is more than two times 110,000 and more than three times 75,000 miles from us. Yet we are able *by naked eye* to see bright projections on the Moon's terminator caused by mountain chains or large craters. When the mountains surrounding *Sinus Iridum* are lying across the terminator, visitors have often asked me for an explanation of the bright projection at that point, seen by them by naked eye. One has only to examine photographs of the Moon at all ages of it to see how prominent the projecting mountains may become. These projections can never be seen on the Moon's bright limb by naked eye. Now, if we can observe prominences on the Moon's terminator at a distance of 240,000 miles *without a telescope*, we ought occasionally to detect similar phenomena on *Mars'* terminator when its equivalent distance in the telescope is less than half the Moon's distance.

To take an actually observed case, let us consider M. PERROTIN's observation of June 10, and assign to the prominence its maximum estimated apparent altitude, $0''.2$. The distance of the planet was at that time about 52,600,000 miles from the Earth. The apparent length of the projection would be $52,600,000 \tan 0''.2$, or 51.0 miles. Assuming the projection to be due to a mountain chain running out upon the non-illuminated surface

of *Mars*, high enough above the surface to catch the Sun's rays while the surrounding plains are not illuminated, the distance of the outer end of the illuminated mountain chain from the terminator would have to be equal to the apparent length of the projection divided by the sine of the angle at *Mars* between the Earth and Sun; that is, to $51.0 \text{ miles} \div \sin 35^\circ = 89.0 \text{ miles}$. The approximate height of a mountain at a distance of 89.0 miles from the terminator, to be just illuminated, would have to be

$$\sqrt{(2100)^2 + (89.0)^2} - 2100 = 1.89 \text{ miles};$$

or a little less than 10,000 feet.

Other things being equal, if the projections are due to mountains, those seen in July would require an altitude somewhat greater than 1.89 miles.

It thus appears that mountains on *Mars*, to be seen under suitable conditions, need be only of moderate height, entirely comparable with those on the Earth and the Moon; and *vice versa*, that if mountains exist on *Mars* of the same order of altitude as those on the Earth and Moon, we ought to be able to see them whenever they are on the terminator and the other conditions are favorable.

If a mountain at the *terminator* becomes visible to us, we do not see it principally by virtue of its *altitude*, as one observer seems to have considered, but almost wholly by virtue of the *length* of the mountain chain under illumination. Due allowance must be made for the fact that the observer is viewing *Mars* from the Earth, and not from the Sun. If a mountain were on the *limb* at a given instant we could see it only by virtue of its *altitude*, and the *length of the chain* would not enter. No projections from the limb were observed here or at Nice, as was stated above.

The mountain chain hypothesis satisfactorily accounts for the more or less permanent character of the projections. They were seen night after night in the same places. At the 1892 opposition they were restricted to the belt between 30° and 50° of south latitude. Possibly they were under suitable illumination in that region, and not in other regions.

There can be no objection to the assumption that the mountains in that region were covered with snow. If the polar caps are composed of snow, we should expect the high mountain tops in all latitudes to be covered with snow at some time in the Martian year. Possibly those in south latitudes 30° to 50° were

covered with snow and were excessively bright in consequence in June and July, 1892. The July projections nearest the south polar cap were brighter than the more distant ones. It should be mentioned that several observers have drawn *Mars* with very bright and narrow spurs jutting out from the polar cap. It is possible or probable that these are mountain ranges from whose tops the snows melt later than they do from the surrounding plains. It is possible also that the Martian mountains are not elevated enough to be visible even on the terminator except when they are made excessively brighter than surrounding areas by snow on their summits. The fact that projections were not equally visible both before and after oppositions, points to some connection with the Martian seasons; as, for example, the melting of the snow on mountain tops as the summer seasons come on.

It has been suggested that the projections may be due to clouds. That theory has some points in its favor; but on the whole it seems unsatisfactory. We are not accustomed to attach as much permanency to terrestrial clouds as these projections have shown. They were observed night after night apparently in the same positions. Those of July 11 and 13, for instance, were visible for two hours, certainly, and probably longer, during which time 800 or 900 miles of the planet passed over the terminator. Those projections were caused by narrow and long objects. They corresponded in position to recognized permanent features of the surface. Bright spots on the surface were observed by SCHIAPARELLI and others to exist for months without sensible change. If SCHIAPARELLI's bright spots, and the bright projections above described were due to clouds, they could probably only be due to clouds resting on cold mountain tops, as they are often seen for days or weeks hanging over terrestrial mountains. But that carries us back substantially to the mountain theory.

It will not be possible to establish any theory until the projections have been accurately observed at several oppositions, or possibly through one or more of the 15-year cycles. The mountain theory is offered here as a working hypothesis.

At the next opposition the position of every projection should be measured as accurately as possible, and its latitude and longitude computed. When that longitude is on the central meridian, the position occupied by the projection can be set off by means of the micrometer, and it ought to be possible to say whether it falls upon a bright or a dark region. The importance

of this method of deciding whether land areas on *Mars* are light or dark is very evident. My accurate measures of July 11 and 13 seem to place the brightest projection upon SCHIAPARELLI'S broad bright region *Noachis*; but a much more extensive series of measures is required to establish the identity satisfactorily.

Great care must be taken in measuring the position of a projection. I would recommend the following method: MARTH'S valuable ephemeris of *Mars* gives the "position angle of the greatest defect of illumination." With the micrometer wires set to that position angle, place the fixed wire tangent to the upper limb and bisect the projection with the movable wire. Again, place the fixed wire tangent to the lower limb and bisect the projection with the movable wire. The diameter of the planet should also be measured, without changing the position angle of the wires. Irradiation caused by the bright polar cap is liable to increase some of the distances measured, especially with small telescopes.

Whatever may be the cause of these interesting prominences projecting out from the terminator, it is clear that the objects observed need not be higher than the mountains on the Earth and Moon.

MOUNT HAMILTON, February 1, 1894.



NOTICES FROM THE LICK OBSERVATORY.

PREPARED BY MEMBERS OF THE STAFF.

THE INNER CORONA OF APRIL 16, 1893.

The frontispiece of the present *Publication* is a direct photocopy of a reduced positive made from the second of the series of eight negatives taken with the 40-foot telescope of the LICK Observatory Eclipse Expedition to Chile.

The reduced copy on glass, made by Mr. COLTON, contains nearly all the detail of the original negative, which is just twice the scale of the illustration; nearly all the finer detail is, however, lost in the illustration here given, which represents the best results so far obtainable with printer's ink. It seems that only positives on glass (and some silver-prints) can do justice to good negatives, and only such glass copies should be studied for special investigations.

In a later publication of the LICK Observatory a silver-print of the faint outer corona will be given, showing the equatorial extensions to a distance of more than 3° , and polar extensions of nearly 2° from the Sun's center.

J. M. S.

LICK OBSERVATORY, March 31, 1894.

LARGE-SCALE PHOTOGRAPHIC MAPS OF THE MOON.

During the year 1890 Dr. LANGLEY, Director of the Smithsonian Institution, sent a circular to various public and private observatories regarding the making of a photographic map of the Moon. This circular is printed in the *Observatory*, 1890, Vol. XIII, pages 403-404. Dr. LANGLEY's circular suggests a scale of $1^{\text{mm}} = 1''$ for such a map, which would make the Moon's diameter about 1.9 metres. Highly successful experiments in enlarging the focal negatives of the LICK Observatory have been

made during the past two years by Baron ALBERT V. ROTHSCCHILD of Vienna, Mr. V. NIELSEN of Copenhagen, and by Prof. WEINEK and DR. SPITALER at Prague ; and these experiments have shown that it is perfectly practicable to produce such a map as Dr. LANGLEY describes by directly enlarging the focal negatives taken at Mount Hamilton. It is also distinctly advantageous to make such enlargements. That is, all features on the Moon whose angular dimensions are small and whose visibility depends upon good definition (as small crater-pits, narrow rills, etc.) are better seen on the enlargements than in any other way. This is entirely true even for the latest enlargements made by Prof. WEINEK, in which the Moon's diameter is about 10 English feet ; and it is not only true for certain selected parts, but it is true for all regions suitably illuminated. Specimens of this work (silver prints) have been sent to various institutions, as the Royal Astronomical Society, the Astronomical Society of France, the Smithsonian Institution, etc., and a set of such prints is now displayed at the Midwinter Fair in San Francisco. An examination of any of these specimens will show the distinct advantage spoken of. This advantage is preserved to a great degree in an autotype reproduction of one of Prof. WEINEK's enlargements (the crater *Tycho*) which is to form a part of Vol. III of the *Publications* of the LICK Observatory, now nearly ready for the press. Those features of the Moon which depend for their visibility upon accenting the faint *contrasts* of the original negatives (as terraces, light streaks, etc.) are not quite so well seen in the autotype plates as they are in the silver prints, or, of course, in the glass. It may now be said that the problem suggested by Dr. LANGLEY's circular is fully solved, and that the desired map can be made.

The only question remaining is what scales will be most useful to astronomers. It is my own opinion that in the first place, an atlas of the Moon to the same scale as MAEDLER's and LOHRMANN's maps (3 Paris feet) is imperatively required. Such an atlas will serve nearly all of the general purposes for which a map of the Moon is commonly required. Owing to the comparatively small enlargement (some 7-fold), the atlas sheets can be made to include quite large areas of the Moon without losing the fainter contrasts of the negatives. Each region of the Moon must be shown under two different illuminations—morning and evening Sun, for example.

It would be very convenient to have a second atlas on the scale of SCHMIDT's great map (6 feet to the diameter) for purposes of comparison. I can see no advantage in changing this scale to the slightly smaller one of $1^{\text{mm}} = 1''$. It is also highly important to have a complete atlas on the largest *practical* scale; *i. e.*, on the largest scale which presents a distinct advantage. This scale is about 10 feet. Prof. WEINEK's prints, above referred to, show known features of the Moon with excellent definition and relief, and they disclose a very great number of new features (*i. e.*, those hitherto unmapped and unphotographed). Nothing is shown, of course, which is not in the original negative; but what is in that negative is made obvious.

It is hoped that the 3-foot and 10-foot atlases will be made in the near future; as soon as the necessary expenses can be provided for. At any rate, the scientific problem is now solved and only the financial one remains.

EDWARD S. HOLDEN.

LICK OBSERVATORY, March 5, 1894.

MISSING MAP OF ARGELANDER'S *DURCHMUSTERUNG* WANTED.

Through the kindness of Professor KÜSTNER, Director of the Observatory at Bonn, the LICK Observatory has received *three* of the missing maps of the *Durchmusterung* mentioned in *Publications A. S. P.*, Vol. VI, page 47, and takes this way of returning sincere thanks for a real favor.

Map No. 2 of the *Durchmusterung* is still lacking in our collection, and we should be glad to purchase it for its weight in silver.

E. S. H.

PUBLICATIONS OF THE LICK OBSERVATORY, VOLS. II AND III.

The second volume of the quarto *Publications* of the LICK Observatory is entirely devoted to observations of double stars, etc., made at Mount Hamilton with the 12-inch and 36-inch equatorials, by Professor S. W. BURNHAM in the years 1888-1892. It is a continuation of his many previous publications of the same kind, and constitutes the most important recent contribution to the subject. The volume will be distributed to the correspondents of the Observatory during the month of April, 1894. The third volume will be sent to the printer in the early summer. It is chiefly devoted to drawings and photographs, enlarged by

Professor WEINEK from negatives of the Moon made at the LICK Observatory. The beautiful illustrations to the volume have been presented to the Observatory by WALTER W. LAW, Esq.

E. S. H.

A PROPOSED ASTRONOMICAL CONGRESS AT THE MIDWINTER FAIR.

A circular proposing an Astronomical Congress to be held in San Francisco during the Midwinter Fair was sent out by Mr. WM. M. PIERSON, Chairman of a Special Committee, on February 10, 1894. It is understood that the project has been dropped, on account of insufficient support. The officers of the LICK Observatory were ready to do their part.

E. S. H.

THE FIRE-BALL OF FEBRUARY 1, 1894.

At 10^h 07^m P. M., my attention was attracted by a bright light towards the east, which proved to be a magnificent fire-ball. It appeared between 35° and 40° above the eastern horizon, in the constellation *Leo*. It swept towards the horizon, inclining towards the north at a small angle, and burst about 10° north of east, amid streamers of very light haze near the horizon, which gave it a weird, violet color, the color being blue when first seen, higher up. Its motion was slow, occupying two or three seconds in its descent, leaving scarcely any train, all traces having disappeared in eight or ten seconds. It is very difficult to properly judge of the light given by these objects, but this was certainly as bright, when it burst, as the full Moon, but not so large. No sound of any explosion was heard. I noted as carefully as possible the point at which it disappeared, and a subsequent determination gave the position of this point as 10° north of east, with probably an error of less than 5°. The position of the point of disappearance is considerably better determined than that of the appearance. This fire-ball was seen at many places in this State and Nevada, and appears to have been one of the most brilliant ever seen on this coast. Information was requested from a number of persons who witnessed the phenomenon, and the following extracts are made from answers received and from voluntary reports :

Professor C. W. TREAT, Napa College, Cal., says: "The meteor was observed at about 10^h 02^m P. M., Pacific Standard

Time (the error may be as much as 2^m). It appeared about 15° or 20° above the horizon, to which it descended almost vertically, passing behind the hill (7° or 8° high) *directly in the east*, as determined by measurement the next morning (error certainly less than 5°). Its motion was slow and probably occupied two seconds, the glow in the heavens lasting some ten seconds after the meteor disappeared. A very strange thing in reference to it is, that at some little distance above the hill its path seemed to widen out for an instant and then contract and widen again just before the meteor disappeared."

Professor C. W. FRIEND, Director Nevada State Weather Service, Carson City, Nev., says: "The meteor was not seen by myself, personally, but by several in this city, but their reports are so conflicting that it is considered best to take the following from Mr. A. M. ASHBY, our voluntary observer at Candelaria, Nev.:

" 'On or about 10^h P. M. (one authority at Columbus says 10^h 05^m P. M., another at this place 10^h 09^m P. M.) a very vivid blue-white light appeared in the south. This light lasted one second or a little more, and was followed in about thirty seconds by a loud explosion which shook hanging lamps and rattled crockery in the racks. The sound of the explosion came from the north. This explosion was immediately followed by a loud roaring, which gradually died away in a north-northeast direction, and which lasted about one minute and forty seconds. This roaring had exactly the same sound as the passage of a shell from a heavy gun has through the air. I did not see the meteor itself, but observed the rest.' "

Mr. JOHN SWINDELL reports from Belleville, Nev., as follows: "About 10^h P. M. a large brilliant meteor passed over within one-quarter of a mile of this place from east to west. I was on the east side of the roundhouse and it appeared to pass directly over, as I had to look straight up to see it. I stepped quickly around the end of the house, about 40 feet, and caught a good glimpse of it just before it disappeared between this station and the mountains. It was immediately followed by a loud explosion."

Mr. F. FORBUSH, Santa Barbara, Cal., reports: "It gave out more light than the full Moon. The color was a bluish pearl. No explosion was heard at this place. It disappeared at a point on the mountains (about 4500 feet high and about 6 miles

distant) north 13 west true meridian, from where I stood on State Street. The meteor moved from the east to the west, but it was so close to the tops of the mountains when I first saw it that it is impossible for me to give the angle at which it struck the earth. The light remained more than two seconds after the meteor disappeared."

Professor JOSIAH KEEP, MILLS College, Cal., reports: "It appeared only a few degrees above the northeast horizon, but it lighted up the landscape in a marvellous manner; I should say it was equal to the brightest moonlight. The whole phenomenon took no more than three seconds. The motion of the meteor was very rapid and nearly vertical to the horizon."

The meteor was also reported by Mr. J. B. PIERCE, 904 Powell Street, San Francisco, as visible about 10^h 5^m P. M., east of Mount Diablo.

Extensive reports were published in the newspapers, from which the following essential points are selected. At Candelaria, Nev., the light was so bright as to cause much excitement, and was followed in thirty seconds by the sound of the explosion, and in three or four minutes by a "loud, crashing noise."

At Carson City, Nev., it was seen in the southeast.

At Belleville, Nev., it was seen in the east about 10^h 10^m P. M., and moved towards the west. "The illumination lasted fully a minute. Near the meteor's path were heard loud explosions about two minutes after its passage, these detonations lasting three minutes. The thermometer fell from 30° to 20° in less than twenty minutes."

At Virginia City, Nev., it passed from the northern to the southern horizon, and was visible from three to five seconds, a hissing sound being heard.

At Keeler, Cal., the meteor was seen to the north.

The explosion was heard at Belleville and Soda Springs, Nev., 6 and 15 miles respectively, north of Candelaria, but not at Hawthorne, 60 miles north, nor Bodie, Cal., 70 miles northwest. At Fish Lake, 30 miles southeast, nothing was heard. At Bishop Creek, Cal., 60 miles southwest, a faint rumbling was heard.

From these observations and reports it is impossible to locate its fall with any degree of accuracy. Professor TREAT's and my own observations show that it disappeared in about the latitude

of Candelaria which accords well with the reports that it passed directly over this point, but these observations are not sufficiently accurate to determine its longitude. Mr. FORBUSH's observations would indicate that it fell about the center of the San Joaquin Valley to the east of the LICK Observatory, but this is improbable. It probably fell in the wild and uninhabited mountains to the west of Candelaria, Nev., and may never be located. Searching parties went out stimulated by the rewards offered, but, up to the present time, no report of any find has been received.

C. D. PERRINE.

MOUNT HAMILTON, March 5, 1894.

A LETTER FROM PROFESSOR NEWTON ON THE CANDELARIA
METEOR OF FEBRUARY 1, 1894.

"MY DEAR PROFESSOR HOLDEN :

"I am very much obliged to you for the advance manuscript copy of Mr. PERRINE's interesting report of observations of the Candelaria meteor of February 1, and for your invitation to add thereto, if I wish, comments of my own. The accounts are conflicting, as Mr. PERRINE says, and they cannot all be satisfied by any assigned path of the meteor. This is usually the case with meteor observations, and attempts to reconcile conflicting statements are often an entire failure. Men frequently combine what they *see* of the meteor path with what they *infer* about it in such manner that we cannot separate the two. But I think some trustworthy and valuable conclusions can be gotten from the accounts so carefully collected by Mr. PERRINE, and the meteor was of such an extraordinary character that it is worth while to deduce all we can from the stories. Allow me first, however, to add thereto an interesting account of the meteor, taken from a letter to me from Mr. CHARLES A. KING of Candelaria. He says :

" ' I will describe what I saw and heard. I live at the railroad depot, a mile from town and 500 feet above it. At 10^h 7^m P. M. (railroad time), February 1, sky clear and cloudless, thermometer 16° above zero, a brilliant white flash of light coming from the west illuminated the whole sky. So bright was it that the flame of a lamp by which I was reading appeared dim as it would in sunlight. As near as I could judge, ten seconds after the flash was a deafening explosion, of a crashing, tearing nature, but very brief. This was followed by a rumbling sound resembling a train.

of cars traveling rapidly, with a regular, intermittent break in the noise, like that caused by a flat place in a car wheel striking the rail each revolution. The rumbling lasted at least two minutes, gradually decreasing in intensity as it passed away to the east. No one about here seems to have seen the approach of the meteor. In the opinion of some people who were out of doors, the color of the flash was a pale blue; others say it was violet; while those inside of buildings unite in declaring it white. Some seconds before we heard the bursting the light had ceased, and all was in darkness.

“‘The symptoms in some respects resembled those of earthquakes. Everything loose, such as dishes, etc., rattled. The house appeared to shake violently, and to rise a little way, say an inch or so, vertically, then come down with a ‘thud.’ But there was none of that wavy motion common to quakes. I did not feel the slight nausea or squeamishness I had always experienced hitherto on such occasions, and a dog I have had for several years which always betrays great uneasiness before an earthquake, and after the shock is over runs about barking loudly, was on that night lying quietly at my feet, but after the explosion cowered away in a corner and trembled violently, and for hours after trembled all over.’

“We may, I think, safely make the following deductions from the several accounts :

“*First.*—The meteor finally disappeared, with explosion, *not far from Candelaria*. The detonations heard at Candelaria, Belleville and Soda Springs, locate the explosion in the vicinity of those places. The hissing sound reported as heard at Virginia City is questionable, inasmuch as the meteor was so distant that no ordinary sound could reach Virginia City till ten or fifteen minutes after the meteor disappeared. At Bishop’s Creek, the faint rumbling, if it came from the meteor at all, implies considerable distance. At most other places no sound was reported.

“*Second.*—The time named for the duration of the interval from the extinction of the meteor’s light to the hearing of the first and loudest explosion measure the distance from Candelaria, Belleville, etc. Sound travels about 12 miles a minute. Mr. KING says the interval was ten seconds, which means a distance of 2 miles from Candelaria. Mr. ASHLEY says it was thirty seconds, which means 6 miles from him to the place of explosion. A newspaper account gives two minutes for the interval at Belle-

ville, which represents a distance of 24 miles, also thirty-six seconds at Candelaria, or 6 miles. The general consensus is that the meteor came down westwardly from those places. I believe that we may safely assume that the sound of the explosion first heard came from a point 5 miles, more or less, westwardly from Candelaria and Belleville. The height is not definitely given by any of the observations, but it may easily have been 2 or 3 miles, or more, from the ground, and all the stories be well represented. I assume that the fact that the explosion was near Candelaria is so well established that all other observed paths must be adjusted to this ending of the luminous path.

"*Third — The meteor was moving from the east*, and was descending at a large angle, both to the vertical and to the horizon. Professor TREAT was about 200 miles due west of the meteor, and its observed path was to him vertical. This limits the path to an east and west plane, and that plane can vary from the vertical only to the admissible limits of error in Professor TREAT'S observation. Mr. PERRINE was 200 miles south 75° west from the place of explosion. I assume that the meteor disappeared to him close to the horizon in the azimuth north 75° east, and that its *apparent* path produced backward, since it came from the direction of *Leo*, was inclined say 10° or 15° to the vertical. The intersection of the vertical path seen by Professor TREAT and this inclined path taken strictly determines the meteor's *quit*. Unfortunately, they do not fix its altitude in the east with desired accuracy. They do show, however, that the meteor was rapidly descending. The *quit* was manifestly considerably above the highest point in the visible paths as seen by Professor TREAT and Mr. PERRINE. But I feel sure that Mr. PERRINE has overestimated the first altitude, which could not have been " 35° or 40° ." Such overestimation of altitudes is usual. The actual altitude at first appearance may safely be assumed as less than 100 miles, and as the body was then $250 \pm$ miles distant the apparent altitude to Mr. PERRINE may be taken as not over 20° to 25° . I think that the two observations, and that of Professor KEEP, imply a *quit* nearly due east, at least as high as 40° .

"On the other hand, Mr. FORBUSH'S statement that the meteor moved, as seen from Santa Barbara, from east to west, implies a considerable angle with the vertical. Mr. SWINDELL'S account clearly requires such motion. Mr. KING says it passed from west

to east, but he was in the house, and may have been misled by the fact that the sounds were first heard in the west but appeared to die away in the east. This inverted order would result from a series of sounds coming from different points of the meteor's path, the more distant sounds coming last.

"*Fourth.*—By reference to a celestial globe, it will be seen that if we draw a line from Lat. $+ 15^\circ$, Long. 125° to Lat. $+ 15^\circ$, Long. 155° the meteor's *quit* may be regarded as not very far from some point in this line. The zenithal attraction will be nearly along this line, and so may be disregarded. The earth's *quit* was in Long. 43° in the ecliptic. The meteor's *absolute quit* would be between its *relative quit* above described and the earth's *quit*, say in the constellation *Gemini*, and 10° or 15° north of the ecliptic. The meteor body was therefore at its descending node, and its orbit had a small inclination to the ecliptic. It had not passed perihelion.

"Yours most truly, H. A. NEWTON."

YALE COLLEGE,

NEW HAVEN, Conn., April 4, 1894.

NEW ASTRONOMICAL STATION FOR HARVARD UNIVERSITY.

CAMBRIDGE, Mass., February 12, 1894.

It was announced this afternoon that on March 1, Harvard University Observatory will start an expedition to Arizona to locate a new observatory station. Such a station has been the plan of Professor PICKERING for years, but he has never before had the necessary financial backing.

PERCIVAL LOWELL of Boston has just donated a large fund for this purpose, and Mr. LOWELL will go as a member of the party. A. E. DOUGLASS, assistant in the Cambridge Observatory, will leave before the other members of the expedition in order to make experiments.

WILLIAM PICKERING, who led the Harvard expedition into Peru, will manage this work.

The station in Arizona will be for visual investigation, as the Harvard station in Arequipa, Peru, is for photographic purposes. This will give the Cambridge Observatory two of the best locations in the world. The great BRUCE photographic telescope now being tested will be shipped to Peru. It will be the largest of its kind in use.—S. F. *Chronicle*.

MEMORANDUM TO FOREIGN CORRESPONDENTS OF THE
ASTRONOMICAL SOCIETY OF THE PACIFIC AND
OF THE LICK OBSERVATORY.

It may be a convenience to our foreign correspondents to give this brief memorandum on the transmission of our foreign exchanges:

(1). If the parcels are small, they should be sent by registered mail, plainly addressed as below:

Astronomical Society of the Pacific,
No. 819 Market Street,
San Francisco, California, U. S. A.

Or,

Lick Observatory,
Mount Hamilton,
California, U. S. A.

(2). If the parcels are too large to be sent through the post-office, and if quick delivery be an object, they should be sent through some one of the parcels-forwarding-agencies, addressed as above with the addition:

Care of B. Westermann & Co.,
812 Broadway,
New York City.

In all such cases, a letter should be addressed to the Astronomical Society of the Pacific, or to the LICK Observatory, with an invoice stating the contents and value of the parcel and the date of its shipment. Without such advice the parcels cannot be passed through the Custom House.

(3). If the parcel is large, it may be forwarded through the foreign agents of the Smithsonian Institution. The address should then read:

Care of the Smithsonian Institution,
Washington, D. C.

In all cases, the parcels should be very carefully packed, and they should be so fastened that they may easily be opened for examination by the Custom authorities, and quickly and safely re-fastened. The addresses should be very plainly written. It is desirable to have the name of the sender on the outside of the parcel as well as a brief description of its contents.

If such parcels are delivered to any one of the following foreign agents of the Smithsonian Institution, they can be forwarded without cost :

Bureau Français des Echanges Internationaux, Paris, France.

DR. FELIX FLÜGEL, No. 1, R. Schumann Str., Leipzig, Germany.

W. WESLEY & SON, 28 Essex St., Strand, London, England.

A complete list of such agents is given in the Report of the Secretary of the Smithsonian Institution for 1893, pages 50, 51.

(4). If the shipment has a value of more than \$100, American money, the invoice must be certified before the American Consul in the city of shipment.

E. S. H.

A BRILLIANT AURORA, FEBRUARY 22, 1894.

An unusually brilliant display of the Aurora Borealis was witnessed pretty generally over the northern coast on the evening of February 22, 1894, between 7 and 8 o'clock. Mr. G. MUECKE, voluntary observer of the Weather Bureau near Aurora Mills, Oregon, sends a sketch in colors and a good description of it as witnessed by him, from which the following is condensed : "At 7:23 P. M. the northern horizon suddenly became a bright yellowish green, and soon, straight, broad rays (never more than four or five at a time) of a very light golden color shot up from just under the *Great Bear*, moving westward, each ray not lasting over 10 seconds ; at 7:30 the sky just above the yellowish green, became a light blood-red extending almost to the altitude of the pole, about the same height as the rays. The whole phenomenon seemed to constantly move westward from its origin a little east of and under the *Great Bear*, disappearing under *Cassiopeia*. The three colors were intensely distinct between 7:35 and 7:45 P. M., Pacific Standard time. No corona borealis was formed, however. After 7:50 the phenomenon faded slowly and ended at 8:07 P. M."

The aurora was reported as being seen at Portland and Cascade Locks, Oregon; Boise, Idaho; Verdi, Nevada; Tacoma, Seattle, and Spokane, Washington, and Wheatland, California. It was seen here at Mount Hamilton for about three-quarters of an hour. The descriptions of it are generally the same.

Professor CAMPBELL says he very often finds the *spectrum* of the aurora in the sky even when the aurora itself is not visible to the eye.

C. D. P.

CONNECTION BETWEEN SOLAR ACTIVITY AND MAGNETIC
DISTURBANCES, ETC., ON THE EARTH [BY MR.

E. WALTER MAUNDER, F. R. A. S.]

In the *Journal* of the British Astronomical Association, 1894, page 83, we find the following succinct statement of observed facts:

Mr. MAUNDER was asked whether the recent extraordinary outbursts of solar activity had been accompanied by corresponding magnetic disturbances and auroræ; and replied "that while the winter of 1891 had been the most prolific in auroræ since 1871, he believed very few had been observed more recently. But with regard to the general connection between sun-spots and magnetic variations, there could be no doubt at all. The curves corresponded closely on the whole, and the mean annual values varied together. But, besides this general correspondence, there was a clear connection between the very greatest solar disturbances and the most violent magnetic storms. * * * * But when we had to do with spots below the rank of the very largest, the connection between them and magnetic storms was not close or clearly traceable at all."

IMPROVEMENT OF THE WATER-SUPPLY AT MOUNT HAMILTON.

The power at Mount Hamilton is derived from the high-service reservoir on the summit of Mount Copernicus (171 feet above the marble floor of the Observatory). This reservoir contains 35,375 gallons, which is not enough water to tide us over the summer months, when there is not sufficient wind to keep the wind-mill at the supply reservoir in operation. Two wooden tanks of 4000 gallons each were added in 1893, and in March, 1894, two more such tanks were placed at the summit of Copernicus. The supply is now 51,000 gallons at the summit, and 60,000 gallons at the lower (wind-mill) reservoir on Huyghens Peak. If all these reservoirs and tanks are full on June 1st, it is probable that we shall have enough water to supply all the present needs for power. Another catchment reservoir for rain-water should be built south of the large dome. It would serve as a fire-protection for the new cottages on Mount Ptolemy and for irrigation of trees and grass. There is enough lost rain-water from the Observatory roofs to fill a very large reservoir every winter.

E. S. H.

THE DAWN OF ASTRONOMY: A STUDY OF THE TEMPLE-
WORSHIP AND MYTHOLOGY OF THE ANCIENT EGYPTIANS,
BY PROFESSOR J. NORMAN LOCKYER, C. B.

[Reviewed by M. W. H.]

The purpose of the present volume is to give an account of the endeavors made by the author during the last three years to see whether any trustworthy conclusions could be reached as to the early astronomical views of the Egyptians from a study of their temples and the mythology connected with their various cults. How Professor LOCKYER came to undertake the investigation is worth recounting from the bearing of the statement on the question of priority of discovery. It seems that when he visited Greece, in March, 1890, he was struck by the curious direction in which the Parthenon was built; and the many changes in direction of the foundations of the temple at Eleusis revealed by the French excavations seemed likewise so suggestive that he thought it worth while to note, by means of a pocket compass, the bearings, so as to see whether there was any possible astronomical origin for the direction of the temple, and the various changes in direction observable. What the author had in his mind, he tells us, was the familiar assertion that, in England, the eastern windows of churches generally face to the place of sun-rising on the festival of the patron saint; this is why, for instance, the churches of St. John the Baptist face very nearly the north-east. This direction toward the sunrising is the origin of the term orientation, which, however, has come to be applied just as frequently to other buildings, the direction of which is toward the west, or north, or south. When Professor LOCKYER returned to England he endeavored to ascertain whether this subject had been worked out, but was led to believe that it had not been discussed in relation to ancient temples, and scarcely even with regard to churches, outside England and Germany. Accordingly, he entered on the inquiry, and remembering that the temples of Egypt had been carefully examined and oriented, both by the French in 1798 and by the Prussians in 1844, he determined to see whether it was possible to get from them any information on the general question. He soon found that, although neither the French nor the German had paid any heed to the possible astronomical ideas of the temple builders, there was little doubt that astronomical considerations had a great deal to do with the direc-

tion toward which these temples faced. In a series of lectures given in November, 1890, he pointed out that, by pursuing this line of investigation, archæologists and others might ultimately be enabled to arrive at dates in regard to the foundation of temples, and possibly to advance knowledge in several other ways. After the lectures were over the author was informed by one of his audience that Professor NISSEN in Germany had published some papers on the orientation of ancient temples. These were at once ordered, but before they were received Professor LOCKYER went to Egypt to make some personal researches on the spot with reference to certain points which it was necessary to elucidate, for the reason that when the orientations were observed and recorded it was not known what use would be made of them, and consequently certain data required for the astronomer's purpose were wanting. In Cairo much interest was taken in the matter by BRUGSCH BEY, who looked up some of the old inscriptions, and one day informed the author that he had found a very interesting one concerning the foundation of the temple at Edfu. From this inscription it is clear that Professor LOCKYER's idea was not new; on the contrary, it was some six thousand years old. Afterward, the author went up the Nile and made some observations which convinced him that, for the orientation not only of Edfu, but of all the larger temples, there was an astronomical basis. Returning to England in March, 1891, Professor LOCKYER received Professor NISSEN's papers, and discovered that a part of the subject of the lectures delivered in 1890 by himself had been previously discussed by Professor NISSEN, who had employed the same materials. It is, therefore, frankly acknowledged by the author that to Professor NISSEN belongs the credit of having first made the suggestion that ancient temples were oriented on an astronomical basis.

The determination of the stars to which some of the Egyptian temples, sacred to a known divinity, were directed, opened a way to a study of the astronomical basis of parts of the Egyptian mythologies. This inquiry Professor LOCKYER has pursued to a certain extent, but he frankly admits that to face it requires an Egyptologist, which he has no pretensions to be. It soon, however, became obvious even to an outsider that the mythology was intensely astronomical, and crystallized early ideas suggested by actual observation of the Sun, Moon, and stars. Moreover, it

became apparent that there were at least two mythologies, representing two schools of astronomical thought. With the view, finally, of obtaining a picture as complete as possible, the author has essayed to bring together the information obtainable from these and other sources, including the old Egyptian calendars, and to compare the results deducible from early Babylonian memorials with those which are to be gathered from the Egyptian myths and temple orientations. After all is said, it is acknowledged by the author that the present state of our knowledge has allowed him to make only a few suggestions, and that, if these inquiries are worth continuing, a good deal of preparatory work has to be done. Of this the most important is a re-survey of the temple sites, with modern instruments and methods. Next should be secured from astronomers tables of the rising and setting conditions of the stars for periods far beyond those which have already been considered. The German Astronomical Society has published a table of the places of a great many stars up to 2000 B. C., but to carry on the investigation begun by Professor LOCKYER it will be necessary to go back to 7000 B. C., and to include southern stars. While the astronomer is doing this, the Egyptologist, on his part, will have to look through the inscriptions with reference to the suggestions which lie on the surface of the inquiry. The astronomical and mythological data want bringing together. The author's own impression is, however, that the work here outlined will not really be so laborious as the statement of it may seem to imply.

A word should be said about the chronology provisionally accepted by the author. The latest date for King MENA, or MENES, the first sovereign of the first Egyptian dynasty is, according to BUNSEN, 3600 B. C.; the earliest date, that assigned by BOECKH, is 5702 B. C.; UNGER, BRUGSCH, and LEPSIUS give respectively 5613, 4455 and 3892. For the purpose of the present inquiry the date taken is 4000 B. C., that is 6000 years ago; this is adopted as the starting point for the long series of remains of various orders to which reference is made in this volume. Now let us glance at Babylonia; here inscribed tablets carry us into the past for a period of certainly 5000 years; but the so-called "omen" tablets indicate that observations of eclipses and other astronomical phenomena had been made for some thousands of years before this period.

What are the conclusions to which Professor LOCKYER's re-

searches seem to point? It results from his investigation of the dawn of astronomy in Egypt that from the earliest times there were astronomical observations carried on, and that practically there were three schools of thought, two of these being original, while the third was an amalgam of the others. To all three schools Sun worship was common, but they may be clearly separated by the associated star worship. There are found in ancient Egypt, as we proceed up the Nile, first worshippers of northern stars, next worshippers of east and west stars, and then worshippers of southern stars. The northern star worshippers are associated with the northeastern corner of the delta; the east and west star cult with the pyramid fields at Gizeh, and the southern star worshippers with upper Egypt. But are we to stop at the places named and acknowledge Egypt to be the true cradle of astronomical science, or do the facts compel us to go a stage further back and to recognize that the true origin was elsewhere; that, in short, astronomy, instead of taking its rise in Egypt, was simply imported thither? As regards the northern star worship, of which there are traces in northeastern Egypt, this might easily have been introduced from Babylonia, where a northern star worship obtained at the epoch required for the hypothesis of an emigration thence to lower Egypt. It must, however, be noted that Babylonia itself was the meeting-ground of at least two different schools of astronomical thought, one of which, corresponding with the worship of southern stars, can only be explained upon the theory that it was brought to Chaldea by emigrants from some region south of the equator, and, therefore, probably east Africa. As it happens, the same worship of southern stars which could only arise south of the equator characterizes the dawn of astronomy in upper Egypt. A little reflection, however, will demonstrate that an astronomical system adapted to a people dwelling south of the equator would not suit a people dwelling north of it, and the latter would, accordingly, have to adapt it to its own climatological and agricultural conditions. The main-spring of earlier astronomy must have been utility, or application to practical needs, and among all primitive peoples the most important times of the year must necessarily have been those connected with seed-time and harvest in each locality. Now the spring equinox and summer solstice south of the equator correspond to the autumnal equinox and the winter solstice north of it. If the colonists who are said to have come to Eridu, at the

head of the Persian Gulf, in very remote times, came from a region south of the equator, they would naturally have brought not only their southern stars but their southern seasons with them. They would at once discover, however, that their spring-time was the northern autumn, and their summer solstice the northern winter. This could go on for a time, and, as a matter of fact it did, but the climatic facts were against such an unnatural system, and eventually the old correspondence of astronomical mythology to actual conditions was brought back by calling the new winter summer, or, in other words, by turning the winter god into the summer Sun god. When, however, the southern star worshippers and the northern star worshippers met in Egypt it was ultimately found that neither the astronomical system based upon the fact of sowing at the autumnal equinox, nor the system adapted to the Euphrates Valley and based upon sowing at the vernal equinox, would suit the condition of things in Egypt, where the rise of the Nile, which took place not at an equinox, but at the solstice, would determine the beginning of the Egyptian year.

What light is thrown by the astronomical evidence brought forward in this book on the history of early Egypt, that is to say upon the interaction and resultant mythology of the north and south star worshippers? The following are the elements of the working hypothesis evolved by Professor LOCKYER from data either ascertained or considered probable. About the year 6400 B. C. there came down the river Nile from the south a swarm of invaders, bearing with them three Moon gods, OSIRIS, THOTH, and KHONSU, and a Sun god, CHNEMU. The invaders found already settled in the valley a population worshipping RA and ATMU. Possibly these aborigines were merely worshippers of the dawn and twilight. The Moon worship was accepted as an addition, and the divine dynasty of OSIRIS began. The swarm of invaders from the south brought with them a lunar year of 360 days, and it was then that the Egyptian calendar beginning I. THOTH commenced. The newcomers built temples at Amada, Semneh, Philæ, Edfu, and probably Abydos. All these were probably OSIRIS temples, so called, because OSIRIS, the Moon god, was the chief deity, and they were used for the determination of the Sun's place at the autumnal equinox, at which time the imported lunar year probably began. About the year 5400 B. C. came a swarm or swarms from the northeast. One certainly came by the Red Sea and founded temples at Redisieh

and Denderah; another may have come over the isthmus and founded Annu. They brought the worship of ANU. Now it was that the divine dynasty of SET was established, and one can imagine religious strife between the partisans of the new northern cult and the southern Moon worshippers. These invaders from the northeast might have come either from north Babylonia, or other swarms of the same race may have invaded north Babylonia at the same time.

We arrive at the date 5000 B. C., a date fixed on one side by the fact that the constellation *Hippopotamus* (now *Draco*) was not circumpolar after it. The date might, in truth, have been much earlier, but could not have been much later. It is at the date thus astronomically fixed that HORUS with his "blacksmiths" comes down the river to revenge his "Father OSIRIS" by killing his murderer, SET (the *Hippopotamus*). The 6400 B. C. people who came from the south had been worsted by the last (5400 B. C.) swarm from the northeast, and had sent for southern assistance. The southern invaders of upper Egypt had by this time become Sun worshippers, and OSIRIS had come to be Sun as well as Moon. In the resultant struggle the northeastern people were beaten, and there was an amalgamation of the cult of the delta and of the southern cult. The northeastern people were reduced to second place, but SET was retained, and ANUBIS looked after sepulchres, but was soon to be replaced by OSIRIS, as southern priestcraft prevailed. The priestly headquarters now were at Annu and Abydos. At the former place arose an amalgamated cult representing the Sun and northern star gods; at Abydos, OSIRIS, changed into a Sun god, was supreme. We reach finally the pyramid times referred by MARIETTE to 4200 B. C. and by BRUGSCH to 3700 B. C. At this epoch arrived another swarm from the northeast, certainly this time from Babylonia, and apparently by the isthmus only, since no east-west temples are found on Red Sea roads. This time they no longer bring ANNU alone, but also a spring equinox Sun god. At Barkal and Thebes, the southern people remain; temple building goes on there on a large scale, and eventually, in 3500 B. C., a thorough blending of north and south cults takes place at Thebes. The worship of AMEN-RA is there established, and Theban priests become supreme.

This is, of course, only the first chapter, and but an outline at that, of a history of ancient Egypt to be evolved from astronomical

data. The author finds it comparatively easy to demonstrate that the astronomical observations and temple worship of the Egyptians form the basis first of Greek, and later of Latin temple building. In Greece, as it was not dominated by the rise of the Nile, one would not expect the year to begin at a summer solstice, but rather at a vernal equinox. The author of this book has shown that, even in pyramid times in Egypt, the risings of the *Pleiades* and *Antares* were watched to herald the equinoctical Sun. It is not surprising, therefore, to find the earliest temples in Greece so oriented. Mr. F. C. PENROSE, who is both an astronomer and an archæologist, has found that, of the ten very ancient temples or temple sites examined by him in Greece, four were directed to the rising and one to the setting of the *Pleiades*, and three to the setting of *Antares* and one to the rising thereof. The Heræum at Argos, the oldest temple in a spot which, by common consent, is the very cradle of Greek civilization, was directed toward the rising of *Antares*. To the question why *Sirius*, which was the paramount star in Egypt at a time when one would expect to find her influence most important in Greece, is not represented among the stars to which Greek temples were built, the answer is that, as the Greek year ignored the solstice, and had reference to the vernal equinox, the use of *Sirius* as a warning star would not be obvious. As a matter of fact, however, Mr. PENROSE has discovered that *Sirius* was used in the great temple of CERES, but simply for its own refulgence at midnight. It is further noteworthy that in two-thirds of the cases examined by Mr. PENROSE in Greece, the dates deduced from the orientation are clearly earlier than the architectural remains now visible above the grounds. This is explained by the temples having been rebuilt upon old foundations, a thing which can be proved to have occurred. In the book before us, Professor LOCKYER has occasion to insist repeatedly that, in the case of the Egyptian temples, the stated date of foundation of a temple is almost always long after that at which its lines were laid down in accordance with the astronomical ritual. No wonder, then, that the same thing is noticed in Greece.—From the *N. Y. Sun*, March 11, 1894.

ERRATUM IN VOL. I, *PUBLICATIONS OF THE LICK OBSERVATORY.*

On page 208, Star No. 434, eighth line of table, column C,
for 1.100 read 1.010.

R. H. T.

NEW PHOTOGRAPHIC TELESCOPE PRESENTED TO THE GREENWICH OBSERVATORY BY SIR HENRY THOMPSON.

SIR HENRY THOMPSON, the eminent surgeon, has, the *Observatory* magazine states, offered the sum of £5000 sterling to the nation, through the Astronomer Royal, for the purpose of buying a telescope for Greenwich Observatory. It is not often astronomy finds such a generous patron, on this side of the Atlantic at least, and, moreover, one who can so well appreciate the exact needs of the science at the moment. For SIR HENRY THOMPSON, foreseeing that the astronomy of the future is to be photographic, and feeling that England should be well equipped in this arm, makes it a condition of his gift that the telescope is to be expressly designed for photographic purposes. So far as the plans are made, and subject to the acceptance of the offer by the Government, the instrument is to be of 26-inch aperture, just twice that of the telescopes used for the photographic chart of the heavens—in fact, the instrument is to be made from the model of the astrographic equatorial, but of exactly double the dimensions in every particular. The guiding telescope for the new instrument will be the 12¾-inch MERZ refractor with a light tube; and the 9-inch photographic objective presented by SIR HENRY THOMPSON to the Royal Observatory some years ago will also be carried on the same mounting, for use as a photo-heliograph, as at present. The new instrument, when completed, will be housed under the LASSELL Dome, on the top of the central octagon of the new Physical Observatory, now being built in the south grounds of the Royal Observatory.—London *Telegraph*, March 3, 1894.

THE SPECTRUM OF LIGHTNING.

An ingenious method of photographing the spectrum of lightning is proposed in the current number of *Wiedemann's Annalen* by G. MEYER. The difficulty of directing the slit of the spectro-scope upon the flash is got over by substituting a diffraction grating for the prism. A grating ruled on glass is placed in front of the object-glass of the apparatus, the object-glass being focused for infinite distances. Under these circumstance several images of the flash are obtained, a central image produced by the undiffracted rays, and images of the first and higher orders belonging to the diffraction spectra. The number of images of each order

corresponds to the number of lines in the spectrum of the lightning. The arrangement was tested during a night thunderstorm. Two plates were exposed in a camera with a landscape lens of 10 cm. focal length, provided with a grating with 40 lines to the mm. One of the plates showed two flashes with their diffraction images of the first order, but representing one line only. The other showed a number of flashes, and one very strong one, passing apparently between two chimney-pots, with its diffraction images well marked. A calculation of the wave-length of the light producing these images gave $382\ \mu\mu$. The measurement was not sufficiently accurate to warrant an identification of this line with a known wave-length, but it is certain that a radiation of about this wave-length must be added to the lines determined by SCHUSTER and VOGEL. It is probable that with better apparatus the method may be made to considerably increase our knowledge of the ultra-violet spectrum of lightning.—*Nature*, March 1, 1894.

SPECIAL STUDENTS AT THE LICK OBSERVATORY.

Applications to be received as special students at Mount Hamilton during the summer of 1894 have been received from the following :

Miss MARY E. EDWARDS Stanford University.

Mr. JOHN S. HODGIN " "

These students have been accepted ; other applications are still under consideration.

STEAM-HEATING IN THE LICK OBSERVATORY.

We have lately fitted some of the offices and rooms of the Observatory with steam-coils formerly used in the South Hall at Berkeley, connecting them with the steam-engine employed to drive the dynamo. In this way it is possible to heat these offices and rooms without being subjected to the intolerable smoke which has been endured—more or less patiently—for six years.* The cost to the Observatory has been *nil*. The coils had been rejected at Berkeley, and the LICK Observatory workmen have done all the steam-fitting.

E. S. H.

* See *Publications A. S. P.*, Vol. II, page 63.

LIST OF EARTHQUAKES IN CALIFORNIA.

The United States Geological Survey has published the following *Bulletins*, containing lists of earthquakes felt on the Pacific Coast :

Bulletin No. 68.—Earthquakes in California in 1889. By J. E. KEELER. 1890. 8vo., 25 pages. Price, 5 cents.

Bulletin No. 95.—Earthquakes in California in 1890 and 1891. By E. S. HOLDEN. 1892. 8vo., 31 pages. Price, 5 cents.

Bulletin No. 112.—Earthquakes in California in 1892. By C. D. PERRINE. 1893. 8vo., 57 pages. Price, 10 cents.

Copies of these *Bulletins* can be had by addressing the Chief Clerk, United States Geological Survey, Washington, D. C. Remittance should be made only by Postal note, money-order, or currency.

C. D. P.

MERIDIAN CIRCLE OBSERVATIONS AT MADISON.

In Vol. VIII of the *Publications of the WASHBURN Observatory*, of the University of Wisconsin, are given the results of the meridian circle observations made in the years 1888 to 1890.

The introduction to the volume, by the Director, GEORGE C. COMSTOCK, gives a compact and finished discussion of all of the details of the methods of observing, and of the reductions. The observations were in charge of Professor S. J. BROWN, U. S. N., who was invited by the Director to go to Madison in order to carry on a series of observations, originally planned for the meridian circle at Annapolis, while the work of the Naval Observatory at Washington was interrupted by the change of location. Professor BROWN, with Mr. ALBERT S. FLINT and Mr. H. V. EGBERT, took part in the observing, two persons being engaged at once.

The plan of work included the determination of the positions of all the "additional stars" of AUWERS' *Fundamental Catalogue*, in terms of the "principal stars" of that system.

Five observations in each position of the instrument were to be obtained; and five additional, in each position, for the circumpolar stars at lower culmination.

All stars south of 60° Declination were used for the Right Ascension determinations, for m and ΔT ; and for azimuth, or n ,

besides the 6 close circumpolars, 9 others were adopted, with small corrections in some cases to the Berlin *Jahrbuch* places.

The level was determined by spirit level. The collimation was obtained at various times by three different methods: by the two collimators directly; by nadir observation, with b from spirit level; and by reversal on the collimators. Preference is given to the second method.

Absolute Declinations were observed, by referring all circle readings to that of the vertical, given by the nadir observation, and assuming the latitude of the Observatory $+43^{\circ} 4' 36''.72$.

The introduction includes the results of the determination of the division error of the 10' diameters of the circle; and it is concluded that they are systematic within a few hundredths of a second.

The graduation was performed by the REPSOLDS, consecutively, from beginning to end.

A discussion of the personal differences between the observers, based upon the results given by each, is included. Also the results for each, of observing in position facing north and facing south. None of the quantities obtained for these small corrections have been applied to the results.

A systematic difference is shown in the observed Declinations of the Berlin *Jahrbuch* stars when compared with that list. This difference changes sign at the zenith, and the lower culmination observations agree with the others in indicating dependence upon the observed zenith distance.

The volume also contains the meridian circle observations of *Mars*, made at the opposition of 1892. R. H. T.

DISCOVERY OF COMET *A*, 1894.

A telegram from the HARVARD College Observatory announces the discovery of a faint comet by Mr. W. F. DENNING of Bristol, England, on March 26.396 G. M. T.

DISCOVERY OF COMET *B*, 1894.

A telegram from Harvard College Observatory announces the discovery of a bright comet by WALTER F. GALE, at Sydney, N. S. W., on April 2.944 G. M. T.

RECENT OBSERVATIONS OF THE SATELLITES OF JUPITER.

In *Astronomy and Astro-Physics* for April, 1894 (page 272), and in the *Monthly Notices*, R. A. S., for January, 1894 (page 134), Dr. BARNARD has two papers on his recent observations of the satellites of *Jupiter*. The following points are especially interesting in connection with previous observations on the same subject:

First.—The remarkable results announced in 1893 by Professor WILLIAM PICKERING with regard to the forms, rotations and behavior of these bodies are not confirmed by Dr. BARNARD. In this respect his conclusions entirely agree with those of Professor SCHAEBERLE, previously printed in the *Publications* A. S. P., Vol. V, page 182, dated September, 1893. In order that Professor PICKERING's conclusions may be accepted, it will be necessary for him to verify the results he obtained in South America during 1892 by renewed observations, with a different object-glass, at his new station in Arizona, or elsewhere, and it seems to be required that some of the large instruments, now so plentiful, should confirm his conclusions.

Second.—In these *Publications*, Vol. III, pages 355 and 359, Professors SCHAEBERLE and CAMPBELL described markings on Satellite III of *Jupiter*, and announced that Satellite I "is ellipsoidal, that its largest axis is directed towards the center of *Jupiter*, and that the other satellites appear to be spherical." Dr. BARNARD's conclusions are that all the satellites, including I, are spherical. Professors SCHAEBERLE and CAMPBELL have continued their observations since 1891, and have, so far, seen no reason to change their conclusions above given, so far as I know. In the same paper they conclude that the periods of axial rotation and of revolution of Satellite I are equal. Dr. BARNARD says that his observations (as yet unpublished) lead to a different result.

Third.—The appearance of Satellite I in transit as a double body, as observed in 1890 by Professors BURNHAM and BARNARD, is now explained by Dr. BARNARD as due to simple contrasts between bright regions on the planet and two extensive dusky polar caps on the satellite (which are separated by a brighter belt).*

The simple theory of contrast is probably fully adequate to explain all observed appearances of the phenomena of the transits of the satellites (dark transits, etc.), as has been pointed out in

* This bright belt was repeatedly observed by Messrs. SCHAEBERLE and CAMPBELL in 1891.

the *Publications* A. S. P., Vol. II (1890), page 296 ; Vol. III (1891), page 358, and in other places.

Fourth.—Dr. BARNARD next considers the transparency of the limb of *Jupiter* with reference to the question whether the light of a satellite undergoing occultation can be seen through the planet's atmosphere, and says "I think it is high time that astronomers reject the idea that the satellites of *Jupiter* can be seen through his limb" since under good conditions, with the 36-inch telescope, "the limb of *Jupiter* has appeared perfectly opaque, as in all my previous observations with smaller telescopes."

It is quite possible that the limb of *Jupiter* is really opaque to the light of satellites or stars, but it does not always appear to be so. Dr. BARNARD has himself described the appearance of a star at occultation shining through the atmosphere of the planet (see *Astronomical Journal*, Vol. VIII, page 64), though the observation had probably escaped his memory when he wrote the sentence just quoted. It is conceivable that the observations of satellites which he criticises were due to the same causes which affected his own observations of the star in question. At any rate his words "the star was last seen with three-quarters of its disc within the limb" of *Jupiter*, show that good observers have sometimes recorded appearances of the kind.

These few points from recent papers show very forcibly that everything is not yet settled with respect to *Jupiter's* satellite system, and may serve to direct the attention of the possessors of large telescopes to some of the problems involved.

April 10, 1894.

E. S. H.

ERRATUM IN *PUBLICATIONS*, NO. 32.

In *Publications*, No. 32, Vol. V., 1893, page 204, the name of the discoverers of Comet *b*, 1893, should have been printed in the order of discovery, thus, SPERRA, ROSO DE LUNA, { MILLER, } RORDAME, QUÉNISSET, instead of in the order in { JOHNSON, } which notices of their discoveries came to the knowledge of the Committee on the Comet-Medal. THE COMMITTEE.

VISITOR TO THE LICK OBSERVATORY.

Hon. BENJAMIN HARRISON, lately President of the United States, and now Lecturer on Constitutional Law at the STANFORD University, visited the Observatory on April 12 and 13.

MERIDIAN CIRCLE OBSERVATION OF COMPARISON STARS.

The three stars which I used for comparison with Comet *b*, 1893, on the nights of July 11, 12 and 13, have been observed with the meridian circle for definite place :

L. O. No.	A. R. 1894.0			Decl. 1894.0			W. B.
	H.	M.	S.	°	'	"	
090646	9	6	22.73	+	44	44 45.3	56
093751	9	37	27.77	+	42	32 16.9	749
095954	9	59	32.98	+	40	5 50.2	1204

The mean epoch of observation is 1894.22. The second star differs — 4" in Declination from BESSEL's place.

Following are the precessions for 1894 :

	s		"	
090646	+	3.967	—	14.57
093751	+	3.787	—	16.30
095954	+	3.637	—	17.35

R. H. T.

THE LICK OBSERVATORY PHOTOGRAPHS OF *MARS*.

It is well known that the border of the disc of *Mars* is considerably brighter than the interior of the disc. Just how much of the observed effect is real and how much is due to contrast with the dark-sky background has been uncertain. The eye likewise is unable to distinguish between small differences of brightness, and it has been supposed that the limb of the planet is about equally brightened at all points of it. The prominent markings on the surface can occasionally be followed nearly to the limb, but they are generally lost to view at some distance from it.

A recent examination of the LICK photographs of *Mars*, taken by Professor HOLDEN in 1890 and 1892, shows that they are extremely valuable in deciding the questions of relative brightness referred to above. The photographs record photographic intensities of light in their true order, and are not affected by brighter or fainter surrounding areas, as must always be the case with eye estimates.

Three series of photographs were taken : with the plate in the focus of the large objective, which gave very small images; with

an intermediate amplifying lens magnifying about 5 diameters; and with an amplifying lens magnifying about 8 diameters.

When *Mars* is *at* or *very near* opposition the photographs show that the border of the planet is very much brighter than the interior areas. But the brightness is far from uniform. When certain dark markings are on the edge of the disc, the limb at those points is only slightly brightened. When certain bright markings are on the edge of the disc, those parts of the limb are almost as bright as the polar caps. The brightness of the periphery is very irregular, and varies as different portions of the planet come to the limb.

When *Mars* is a few weeks from opposition the *limb* is irregularly brightened, just as described above, and the terminator is similarly brightened, but to a less extent.

When *Mars* is at a considerable distance from opposition, so that the terminator shows plainly, and the planet is gibbous, the *limb* is brightened as before, but the *terminator* is not perceptibly brightened. The terminator is very irregular in outline and is nearer the center of the disc than its theoretical position, just as in the case of Moon photographs.

The photographs show the most prominent surface features of the planet very clearly.

It has been suggested that the increased brightness of the limb of *Mars* is produced by the planet's *atmosphere*. It seems to me that the irregularities and variations in the brightness of the limb render that view untenable. If the effect were wholly atmospheric we would expect the limb to be uniformly brightened.

Several astronomers have suggested that the increased brightness is due to morning and evening clouds. If that were true, the *limb* should not be brightened perceptibly when the angle at *Mars* between the Sun and Earth is large. Some of our photographs were made when that angle was $37^{\circ}.5$, on May 31, 1892. We would expect the morning clouds to be completely hidden from us by the planet at that time. Yet the photographs show no perceptible diminution in the usual brightness of the limb. The very low albedo of the planet is also an objection to the cloud theory.

The photographs show pretty conclusively that *the brightened border is caused by surface features of the planet, the brightest regions giving the brightest borders*. The same effect is well known in the case of the Moon; the limb is brighter than the

interior because the mountain structures reflect more light than the level areas; at the *limb* the mountains form the entire visible reflecting background, the level plains and valleys not forming any part of it. I think we must look for a similar explanation in the case of *Mars*; though, as others have pointed out, other things being equal, very steep mountain slopes would be required.

The importance of making a series of short-exposure photographs in connection with other observations of the planet is very evident.

W. W. C.

THE LEVELS OF THE MERIDIAN CIRCLE.

A progressive change has been taking place in the level of the instrument, marked by exceptional uniformity with but little variation from day to day.

It seemed at first possible to connect the change with that of temperature, as the level correction decreased with the approach of cold weather. But whatever small part of the change follows from this cause is evidently masked by the regular movement. The fluctuations of temperature within periods of a few days have often been nearly up to the total range, while the level has shown no variation to correspond.

In tabulating the mean level by months, the temperature given is the mean of the thermometer readings at the times the levels were taken :

		<i>δ.</i>	Temp.
1893.	September	+ .663	54° .5
	October	+ .648	55 .2
	November	+ .623	50 .6
	December	+ .561	48 .8
1894.	January	+ .506	45 .3
	February	+ .402	40 .3
	March	+ .359	44 .3

The azimuth of the instrument shows no progressive change, the daily fluctuations being nearly as great as the whole variation.

R. H. T.

SCHIAPARELLI'S OBSERVATIONS OF BRIGHT SPOTS ON *MARS*.

On a previous page of this *Publication* I referred to SCHIAPARELLI'S observations of bright spots on the planet *Mars*. I have translated his account of them as follows, from *Himmel und Erde*, Vol. I, pages 14, 15, 16, 159, for the benefit of *Mars* observers.

W. W. C.

“The marked peculiarity that the regions of doubtful character are often brighter in oblique positions near the edges of the planet than at the central meridian, applies also to some regions of a purely continental character. We may mention particularly as of this character the two nearly circular regions, *Elysium* and *Tempe*. Very frequently these regions would be white with a more or less bright luster, but always less brilliant than the polar cap. But their white color is generally seen best when they are near the edge of the disc, and I have often observed them even when, some hours earlier or later, on passing over the central meridian, they have shown nothing unusual. The similar transformations of *Argyre Island* are especially interesting. At times, when on the edge of the planet, it has been so brilliant that some observers have actually taken it for a polar cap. This intensely bright island was first observed by DAWES, in 1852, and English observers of *Mars* have called it DAWES' *Snow Island*. Yet I have often seen it of a yellow or even dark-red color when near the central meridian. I consider another island, which is smaller and further south, to be of a similar nature. It is called on the map *Argyre II*. I first saw it on November 8, 1879, when it was on the left edge. It was only a little less bright than the polar cap, but, when passing over the central meridian, it was very much fainter and of a dull-red color.

“In addition to these changes of color which depend upon the diurnal rotation of the planet, we observed in the continental regions still other similar changes, but of slower character, which often embraced very extensive regions. Thus, for example, in the years 1877-79 the whole extensive region lying under *Mare Sirenum* between longitudes 120° and 170° down to 40° North Declination was far brighter than the other continental districts, especially the upper part bordering upon the above mentioned sea. Traces of dark markings were unusually indefinite and hard to make out. In the year 1882 this part appeared more yellow, and it was possible (although requiring great effort) to see a complicated system of dark lines, as also (though less completely) in the years 1884 and 1886. But in 1888 this region was again bright and white, and it was with difficulty that a trace of the lines observed at preceding oppositions could be seen.

“To this belong also the observations made by me in 1877-82 of a small, bright white spot which was situated at the left end of *Nepenthes* at longitude 269° and latitude $+17^{\circ}$. I first saw this

spot on September 14, 1877, when it was almost square and about 8° on a side. It was more brilliant than any other part of the planet, its borders were well defined, and I have no hesitation in saying it was comparable in brightness to the south polar cap. It was still visible on the 14th of October. I observed the same phenomenon at the same place at the following opposition, from November, 1879, to January, 1880. Its size had not changed, but its form had become almost round. Astonished at its persistency, I gave it the name *Nix Atlantica*. At the opposition of 1881-82 I saw it again, from November to March, but not always with equal ease; it showed differences of appearance and fluctuations in brilliancy which perhaps could not always be attributed to the various conditions of the telescopic images. But at the following oppositions I have sought for it in vain, and it has also been invisible this year (1888). If its appearance depends upon the time of the year on *Mars*, then we should expect to see it again at the 1892-97 oppositions, and it is easy to realize how valuable its reappearance would be in the investigation of the physical constitution of the planet.

"Another similar spot (*Nix Olympica*), though much smaller and more difficult, was very persistent during the 1879 opposition in the position longitude 129° and latitude $+21^\circ$. Its diameter could not have been more than 4° . It has not been seen at other and later oppositions. Other spots of more or less brilliant and pure white have been seen here and there in the different continental regions, generally several days in one place, and following no apparent law. They have been seen very often during the last opposition along the straight shore of *Syrtis Major* and along the coast from there to *Sabaeus Sinus*, as well as in many other parts. * * * Often a remarkably large part of the planet is sprinkled with white spots, as, for example, on the 18th and 19th of January, 1882, in the region between the *Ganges* and *Iris*, and January 31, 1882, between *Nilosyrtis* and *Indus*.

* * * * *

"It would not be difficult to find a series of hypotheses which would explain satisfactorily the appearance of the polar and other white spots by attributing them in some way to the evaporation of the supposed seas, and to the atmosphere of the planet whose existence is indisputable. But I consider it more useful to point out that these different white spots are, of all the species of appearances on *Mars*, the easiest to observe. They require only an

instrument of moderate power and a very persevering attention. The peculiarities which I have pointed out concerning these spots show that they offer a field for the most interesting investigations, whose importance in the study of the physical constitution of *Mars* is obvious; and in this field useful work could be done by those observers who are not able to decipher the much more difficult details of the canals and their doubling."

DISCOVERY OF COMET I, 1893.

In *Astronomy and Astro-Physics* for April, 1894, page 307, Professor SCHAEBERLE has described a cometary form which he found on his eclipse negatives taken in Chile on April 16, 1893, and to which he first called attention at the World's Fair Astronomical Congress in August, 1893. In May, 1894, we received copies of the eclipse negatives taken by the British expeditions to Brazil and Africa, respectively. Professor SCHAEBERLE has found the same object on the British plates also. The distance of the brightest part of the comet from the Moon's limb, expressed in fractions of the Moon's diameter, is: Chile, 0.88; Brazil, $1.15 \pm$; Africa, $1.50 \pm$. The perihelion passage of this comet must be about April 16. The daily geocentric motion of the comet at the time was about $3\frac{1}{4}$ degrees.

The comets of 1893 should therefore be numbered:

Comet I. Discovered by SCHAEBERLE;

Comet II. Discovered by $\left\{ \begin{array}{l} \text{SPERRA, R. DE LUNA,} \\ \text{RORDAME and others;} \end{array} \right.$

Comet III. Discovered by FINLAY;

Comet IV. Discovered by BROOKS.

EDWARD S. HOLDEN.

LICK OBSERVATORY, 1893, May 8.

MINUTES OF THE MEETING OF THE BOARD OF DIRECTORS,
HELD IN THE ROOMS OF THE SOCIETY, MARCH 31,
1894, AT 7:30 P. M.,

President MOLERA presided. A quorum was present. The minutes of the last meeting were read and approved. The following members were duly elected:

LIST OF MEMBERS ELECTED MARCH 31, 1894.

Mrs. WM. ASHBURNER	1014 Pine Street, S. F., Cal.
Mr. EMIL R. GRAEBEIEL	Elm Creek, Buffalo Co., Neb.
Prof. ASAPH HALL, JR.	{ The Observatory, Ann Arbor, Michigan.
Mr. GEO. RUSSELL LUKENS	1350 Madison St., Oakland, Cal.
Dr. GEORGE W. MERRITT	{ Care Munroe & Co., 7 Rue Scribe, Paris, France.
Miss JULIET PORTER	{ 11 E. Shelby Street, Worcester, Mass.
Mr. LOUIS C. PORTERFIELD	Tidioute, Warren Co., Penn.
Mr. DANIEL S. RICHARDSON	{ Cor. Lincoln and Summit Sts., East Oakland, Cal.

Adjourned.

ANNUAL MEETING OF THE ASTRONOMICAL SOCIETY OF THE
PACIFIC, HELD IN THE LECTURE-HALL OF THE CALI-
FORNIA ACADEMY OF SCIENCES, MARCH 31, 1894.

The meeting was called to order by President MOLERA. The minutes of the last meeting were read and approved.

The Secretary read the names of new members duly elected at the Directors' meeting.

The following papers were presented :

1. Annual Address by the retiring President E. J. MOLERA.
2. The CHABOT Observatory, by CHAS. BURCKHALTER.
3. Can Organic Life exist in the Planetary System outside of the Earth? by C. A. STETEFELDT.

The Committee on Nominations reported a list of names proposed for election as Directors as follows: Messrs. ALVORD, CAMPBELL, DOLBEER, HOLDEN, HUSSEY, MOLERA, PERRINE, PIERSON, SCHAEBERLE, VON GELDERN, ZIEL.

For Committee on Publication: Messrs. HOLDEN, CAMPBELL, YALE.

Messrs. STETEFELDT and BABCOCK were appointed as tellers. The polls were open from 8:15 to 9 P. M., and the persons above named were duly elected.

REPORT OF THE COMMITTEE ON THE COMET-MEDAL,
SUBMITTED MARCH 31, 1894.

This report relates to the calendar year 1893. The comets of 1893 have been:

Comet *a* (FINLAY's periodic comet); rediscovered by W. H. FINLAY, at the Cape of Good Hope, on May 17.

Comet *b* (unexpected comet); visible to the naked eye, discovered by various observers, at various places, in June and July.

Comet *c* (unexpected comet); discovered by W. R. BROOKS, at Geneva, New York, on October 16, 1893.

The Comet-Medal has been duly transmitted to the discoverer of Comet *c* in accordance with the regulations.

DISCOVERY OF COMET *B*, 1893.

Notes on the independent discovery of this comet by Messrs. SPERRA, ROSO DE LUNA { MILLER, } RORDAME, QUÉNISSET have been printed in these *Publications*, 1893, pages 154-5. A full account of Mr. SPERRA's observations is given in *Astronomy and Astro-Physics*, 1893, page 757. The Committee on the Comet-Medal, having carefully considered the case, and having asked the advice of the editors of the leading astronomical journals, adopted the following resolutions:

I. That a copy of the Comet-Medal shall be struck, having the obverse as usual and the reverse blank, and that on the reverse of this copy shall be engraved the words:

TO COMMEMORATE
THE
DISCOVERY OF COMET *B*,
1893.

II. That this medal shall be preserved in the cabinet of the Astronomical Society of the Pacific, and no award made for the discovery of this comet.

III. That a copy of No. 32 of the Society's *Publications* shall be sent to each of the gentlemen named above.

These resolutions have been carried out.

Respectfully submitted,

EDWARD S. HOLDEN,
J. M. SCHAEBERLE,
CHAS. BURCKHALTER.

The Treasurer read his Annual Report as follows :

ANNUAL STATEMENT OF THE RECEIPTS AND EXPENDITURES OF THE
ASTRONOMICAL SOCIETY OF THE PACIFIC FOR THE
FISCAL YEAR ENDING MARCH 31, 1894.

GENERAL FUND.

Receipts.

Cash Balance March 25, 1893		\$ 593 33
Received from dues	\$1,981 85	
" " sale of publications.....	67 04	
" " sale of stationery.....	50	
" " advertisements.....	30 00	
" " Life Membership Fund (interest).....	57 47	
" " Security Savings Bank (interest).....	73	
	\$2,137 59	
Less transfer to Life Membership Fund.....	350 00	1,787 59
		<u>\$2,380 92</u>

Expenditures.

For publications.....	\$ 900 15	
" general expenses.....	1,102 38	\$2,002 53
Cash Balance March 31, 1894		378 39
		<u>\$2,380 92</u>

LIFE MEMBERSHIP FUND.

Cash Balance March 25, 1893	\$1,100 61	
Received from General Fund.....	350 00	
" " interest.....	57 47	
	\$1,508 08	
Less Interest transferred to General Fund.....	57 47	
Cash Balance March 31, 1894		<u>\$1,450 61</u>

DONOHUE COMET-MEDAL FUND.

Cash Balance March 25, 1893	\$ 585 19	
Interest.....	29 59	
Cash Balance March 31, 1894.....		<u>\$ 614 78</u>

ALEXANDER MONTGOMERY LIBRARY FUND.

Cash Balance March 25, 1893.....	\$1,714 01	
Interest.....	87 54	
	\$1,801 55	
Expended for books.....	85 00	
Cash Balance March 31, 1894.....		<u>\$1,716 55</u>

FUNDS.

General Fund. Balance on deposit with Grangers Bank.....	\$327 66	
" " Balance on deposit with Security Savings Bank....	50 73	\$ 378 39
Life Membership Fund. Balance on deposit with San Francisco Savings Union.....		1,450 61
Donohue Comet-Medal Fund. Balance on deposit with San Francisco Savings Union.....		614 78
Alexander Montgomery Library Fund. Balance on deposit with San Francisco Savings Union.....	\$ 837 27	
Alexander Montgomery Library Fund. Balance on deposit with German Savings and Loan Society.....	879 28	1,716 55
		<u>\$4,160 33</u>

SAN FRANCISCO, March 31, 1894.

F. R. ZIEL, *Treasurer.*

The Committee appointed to audit the Treasurer's accounts reported as follows, and the report was, on motion, accepted and adopted, and the Committee discharged :

To the President and Members of the Astronomical Society of the Pacific:

GENTLEMEN—Your Committee appointed to audit the accounts of the Treasurer for the fiscal year ending March 31, 1894, have made a careful examination and find same to be correct.

(Signed)

E. K. CHAPMAN,
J. A. LIGHTHIPE.

SAN FRANCISCO, March 31, 1894.

Mr. MOLERA then read his Annual Address.

The thanks of the Society were returned to the CALIFORNIA ACADEMY OF SCIENCES for the use of their lecture-hall. Adjourned.

MINUTES OF THE MEETING OF THE DIRECTORS OF THE
ASTRONOMICAL SOCIETY OF THE PACIFIC, HELD
IN THE ROOMS OF THE SOCIETY, MARCH
31, 1894, FROM 9:45 TO 10 P. M.

The new Board of Directors was called to order by Mr. MOLERA. A quorum was present. The minutes of the last meeting were approved.

The business in hand being the election of officers for the ensuing year, the following officers were duly elected :

President: Mr. W. W. CAMPBELL.

First Vice-President: Mr. W. J. HUSSEY.

Second Vice-President: Mr. WM. M. PIERSON.

Third Vice-President: Mr. JOHN DOLBEER.

Secretaries: Messrs. C. D. PERRINE and F. R. ZIEL.

Treasurer: Mr. F. R. ZIEL.

The President was authorized to appoint the various Standing Committees of the Directors, and accordingly made the following selections :

Finance Committee: Messrs. PIERSON, DOLBEER, VON GELDERN.

Library Committee: Messrs. VON GELDERN, MOLERA, BABCOCK.

The Standing Committees of the Society are :

Committee on Publication: Messrs. HOLDEN, CAMPBELL, YALE.

Committee on the Comet-Medal: Messrs. HOLDEN (*ex-officio*), SCHAEERLE, BURCKHALTER.

Adjourned.

OFFICERS OF THE SOCIETY.

W. W. CAMPBELL (Lick Observatory), ... *President*
 W. J. HUSSEY (Leland Stanford Jr. University, Palo Alto, Cal.), }
 WM. M. PIERSON (Mills Building, S. F.), } *Vice-Presidents*
 JOHN DOLBEER (10 California Street, S. F.), }
 C. D. PERRINE (Lick Observatory), ... *Secretary*
 F. R. ZIEL (410 California Street, S. F.), ... *Secretary and Treasurer*
Board of Directors—Messrs. ALVORD, CAMPBELL, DOLBEER, HOLDEN, HUSSEY, MOLERA,
 PERRINE, PIERSON, SCHAEFERLE, VON GELDERN, ZIEL.
Finance Committee—Messrs. PIERSON, DOLBEER, VON GELDERN.
Committee on Publication—Messrs. HOLDEN, CAMPBELL, YALE.
Library Committee—Messrs. VON GELDERN, MOLERA, BABCOCK.
Committee on the Comet-Medal—Messrs. HOLDEN (*ex-officio*), SCHAEFERLE, BURCKHALTER.

OFFICERS OF THE CHICAGO SECTION.

Executive Committee—RUTHVEN W. PIKE.

OFFICERS OF THE MEXICAN SECTION.

Executive Committee—CAMILO GONZALEZ, FRANCISCO RODRIGUEZ REY, AGUSTIN ARAGON.

NOTICE.

The attention of new members is called to Article VIII of the By-Laws, which provides that the annual subscription, paid on election, covers the *calendar* year only. Subsequent annual payments are due on January 1st of each succeeding calendar year. This rule is necessary in order to make our book-keeping as simple as possible. Dues sent by mail should be directed to Astronomical Society of the Pacific, 819 Market Street, San Francisco.

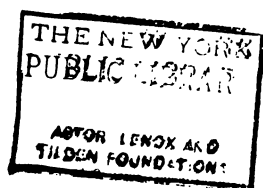
It is intended that each member of the Society shall receive a copy of each one of the *Publications* for the year in which he was elected to membership and for all subsequent years. If there have been (unfortunately) any omissions in this matter, it is requested that the Secretaries be at once notified, in order that the missing numbers may be supplied. Members are requested to preserve the copies of the *Publications* of the Society as sent to them. Once each year a title-page and contents of the preceding numbers will also be sent to the members, who can then bind the numbers together into a volume. Complete volumes for past years will also be supplied, to members only, so far as the stock in hand is sufficient, on the payment of two dollars to either of the Secretaries. Any non-resident member within the United States can obtain books from the Society's library by sending his library card with ten cents in stamps to the Secretary A. S. P., 819 Market Street, San Francisco, who will return the book and the card.

The Committee on Publication desires to say that the order in which papers are printed in the *Publications* is decided simply by convenience. In a general way, those papers are printed first which are earliest accepted for publication. It is not possible to send proof sheets of papers to be printed to authors whose residence is not within the United States. The responsibility for the views expressed in the papers printed rests with the writers, and is not assumed by the Society itself.

The titles of papers for reading should be communicated to either of the Secretaries as early as possible, as well as any changes in addresses. The Secretary in San Francisco will send to any member of the Society suitable stationery stamped with the seal of the Society, at cost price, as follows: a block of letter paper, 40 cents; of note paper, 25 cents; a package of envelopes, 25 cents. These prices include postage, and should be remitted by money-order or in U. S. postage stamps. The sendings are at the risk of the member.

Those members who propose to attend any or all of the meetings at Mount Hamilton during the summer should communicate with "The Secretary Astronomical Society of the Pacific" at the rooms of the Society, 819 Market Street, San Francisco, in order that arrangements may be made for transportation, lodging, etc.







VASSAR COLLEGE OBSERVATORY.

PUBLICATIONS
OF THE
Astronomical Society of the Pacific.

VOL. VI.

SAN FRANCISCO, CALIFORNIA, JUNE 9, 1894.

No. 36.

VASSAR COLLEGE OBSERVATORY.

BY PROFESSOR MARY W. WHITNEY.

Vassar College Observatory was established and equipped at the opening of the college in 1865.

The equatorial telescope has an objective of $12\frac{1}{3}$ inches aperture and a focal length of $16\frac{3}{4}$ feet. It was originally made by FITZ of New York, but in 1872 the glass was re-cut by CLARK, and in 1888 the telescope was re-mounted by WARNER & SWASEY. It was also at that time provided with electrical illumination for the filar micrometer. The magnifying powers, negative and positive, range from 150 to 600. A spectroscope made by BRASHEAR was added in 1890. This spectroscope has a prism for star spectra and a ROWLAND grating for the solar spectrum. There is also a BROWNING direct-vision spectroscope. The transit circle has an objective aperture of $3\frac{3}{4}$ inches. It was made by YOUNG of Philadelphia. In 1889 it was re-mounted by SAEG-MULLER. The clock and chronograph are of BOND manufacture.

Beside the mounted instruments, the observatory has two portable telescopes of 3 and 6-inch apertures, both made by CLARK; also a universal instrument from WANSCHAFF of Berlin. The instruments are used almost altogether for purposes of instruction. There are three classes in each college semester. The portable telescopes are exclusively devoted to class uses, and the spectroscope is at present (1894) serving the needs of a class of seven in solar physics, who are required to themselves observe the phenomena discussed in the lecture. The second-year-students in astronomy have practical instruction with both the meridian and equatorial instruments. Therefore only such occasional

independent observations can be made as will not interfere with the regular duties of the department. A few articles on comet places, features of *Jupiter* and the new star in *Auriga*, have been published during the past two years.

The meteorological equipment consists of maximum and minimum thermometers and rain-gauge. Records are kept throughout the year and monthly reports are sent to the New York Meteorological Bureau.

The first Director of the observatory was MARIA MITCHELL, of Nantucket, who took charge of the department at the opening of the college and retained the professorship for twenty-three years.

The final determination of the latitude of the observatory was secured by Miss MITCHELL in 1872 by a zenith telescope loaned by the U. S. Coast Survey, and that of longitude in 1877 by telegraphic connection with Harvard College Observatory.

ASTRONOMICAL PHOTOGRAPHS AT THE MID-WINTER FAIR.

BY ALLEN H. BABCOCK.

The LICK Observatory exhibit at the World's Fair of 1893, in Chicago, was composed of about 100 specimens of the photographic work of various members of the staff of astronomers. It consisted chiefly of framed transparencies on glass and was placed in the exhibit of Santa Clara County in the California building. The location was an unfortunate one in some respects, for few who are interested in the subject would think of looking for an astronomical exhibit in the midst of a collection of fruits and cereals. The photographs were moved to San Francisco at the close of the World's Fair, are now on exhibition in the Santa Clara County building at the Midwinter Fair, and later will be presented to the Chicago Academy of Sciences.

The Regents were determined that the University should be properly represented at the Midwinter Fair and to that end made an appropriation of \$5000 for a University exhibit. As a whole, the exhibit of the University of California is very dignified and appropriate. It occupies the whole of the north gallery of the

Liberal Arts building, and consists of exhibits which are intended to illustrate the work of its various departments—those of arts, pure science, of engineering, mines, mechanics, metallurgy, agriculture, etc.,—which now contain some 1400 students, directed by some 180 professors and instructors. Two hundred dollars was assigned to the LICK Observatory as its share of the appropriation. This sum was scarcely sufficient to pay for the negative plates used, and would have been entirely inadequate to make a proper exhibit at San Francisco, if it had not been that the Observatory had begun its preparation several months before and had already made considerable progress when the Regents decided to make a University display. The result is worthy the attention of all those interested in the science, in one of the most important applications of photography, and in the great Observatory itself. The Observatory exhibit at San Francisco consists of some 250 transparencies on glass, usually 8 x 10 inches in size, which are copied from the original negatives preserved at Mount Hamilton. They are hung along and over the rail of the gallery and are readily examined by those who are passing. Each photograph bears a label which gives a short account of the subject of the picture, the name of the photograph, etc.

Modern study of the Sun is illustrated by the eclipse pictures of January, 1889, December, 1889 and April, 1893, especially by the incomparable negatives of the latter eclipse made by Professor SCHAEBERLE in Chile. It will require months of study to develop the mine of information contained in them. The results so far obtained tend to substantiate the mechanical theory of the solar corona, as has been announced already. A peculiarly interesting feature of these negatives is that they show upon them the figure of a large comet which was passing close to the Sun at the moment of the eclipse. When Professor SCHAEBERLE first announced this fact, it was evidently regarded with doubt by the other eclipse observers, who had also made negatives of the same eclipse. In the course of time copies of the British negatives, taken in Brazil and in Africa, came to Mount Hamilton, and Professor SCHAEBERLE at once found on them the eclipse comet, thus proving, at the same time, the fact of the existence of the comet and his own skill as a keen observer. This is the second comet which has been photographed at an eclipse, though it is far fainter than the comet of 1875, which was visible to the naked eye.

Trials have shown that the atmospheric conditions at Mount Hamilton are not favorable to regular observations in the daytime, on account of the disturbed conditions of the air at the top of the mountain; but the admirable negatives of the Sun by Mr. C. D. PERRINE, prove conclusively that by persistent watching for favorable moments, as good results can be obtained in this as in other lines of work. The enlargements of these negatives by Mr. A. L. COLTON afford an opportunity for the study of the details of Sun-spots. The partial eclipse of October, 1893, was also photographed by Mr. PERRINE, but the results are somewhat unsatisfactory on account of the cloudy weather prevailing almost the entire day.

The photographs of *Jupiter* taken, 5 and 8 times enlarged, with the great telescope, are extremely fine. They are the work of Professor HOLDEN. One of these has been again enlarged in the camera by Mr. COLTON to 30 diameters. The series of *Jupiter* negatives constitute a very satisfactory history of the planet since they were begun in 1890.

Up to the present time it has been impossible to secure good photographs of *Mars*, on account of its low altitude. The planet is so small and the polar caps so bright, that if sufficient length of exposure is given to bring out the details on the disc, the halation effects at the poles, etc., produce a distorted image. It is hoped that good negatives will be secured at the coming opposition when the altitude of the planet will be considerable. A set of photographs of the elaborate series of drawings of *Mars* made at the opposition of 1892 with the 36-inch telescope are especially noteworthy. It is interesting to compare the LICK Observatory drawings, which are full of detail, with the drawings made at the Arequipa Observatory at the same opposition.

As might be expected from his record as a comet-seeker, Professor BARNARD has sent some beautiful specimens of his work on comets, nebulae and the Milky Way. These last named are extremely beautiful and possess at least one quality seldom met with in celestial photography. Professor BARNARD has succeeded in producing photographic images that present nebulous cloud forms similar to those presented to the eye by the originals in the sky. In the case of the comet negatives, their value arises from diametrically opposite reasons. The human eye is so imperfect a piece of apparatus, that it cannot record the delicacies of comet structure even when aided by the most perfect telescope

in existence. It cannot pay attention long enough. The result is that a photographic plate, if exposed for a sufficient length of time, will present an appearance quite different from that which the comet presents to the eye when viewed with the same instrument which was used in taking the photograph. The negatives of SWIFT'S comet of 1892 are particularly worthy of mention in this regard. They show the most remarkable changes in structure, especially of the tail,—changes which took place with such unusual rapidity, that the accuracy of the observations might be called in question, were it not for the permanent photographic records. Many of the changes in details, in fact the existence of the details, might never have been discovered except by aid of the photographic plate. The photographs of Comet *B*, 1893, by Professor HUSSEY, and of BROOKS' and GALE'S comets by Professor BARNARD, disclose similar features. Professor BARNARD'S discovery of a comet on one of his Milky Way plates of October 12, 1892, stands unique as the first discovery of a comet by purely photographic means. The object is so minute, and the plate so completely covered by star images, that it required a skilled and critical observer to detect anything unusual in the appearance of the plate. His negatives of the *Andromeda*, WOLF and SWIFT nebulae, are especially good, those of the trails of the *Hyades*, *Orion* and the circumpolar stars are interesting as illustrating the apparent motion of the stars, while those of the various instruments of the LICK Observatory, interiors of the buildings, snow scenes and landscapes in the neighborhood of the Observatory, add to his reputation as a skilled photographer.

Any account of the nebula transparencies would be incomplete without mention of the splendid specimens secured by Professor SCHAEBERLE on his eclipse expedition to Chile in 1893. All of these nebulae and star clusters are invisible at this latitude. In addition to the above, he has sent several photographs of *Nova Aurigæ*, which are part of a series giving a complete account of its changes of photographic magnitude.

Recent developments in stellar spectroscopic work are illustrated by the admirable photographs of Professor CAMPBELL. All these photographs have been taken with a visual spectroscope attached to the 36-inch equatorial and temporarily adjusted for photography, a combination not the most advantageous for this class of work. Mr. D. O. MILLS of New York has given a new photographic spectroscope, which has just been completed

by Mr. BRASHEAR, after Professor CAMPBELL's designs. With this new instrument it will be comparatively easy to carry out the spectroscopic work of the Observatory, which has so far been performed under serious disadvantages. Professor CAMPBELL's photographs of the spectra of *Nova Aurigæ* and of the later comets, form an instructive collection. His landscapes, notably one of the big fires of 1891 near Mount Hamilton, views near and around the Observatory and the University of California, are beautiful specimens of the photographic art.

The large collection of Moon photographs is composed of originals made with the 36-inch telescope by Professor HOLDEN, and of enlargements of them made by Mr. COLTON, Baron VON ROTHSCHILD, Mr. NIELSEN and by Dr. SPITALER and Professor WEINEK. These enlargements show in minute detail the principal lunar formations on a large scale, and are to form parts of a complete atlas of the whole Moon. The later prints by Professor WEINEK are enlarged 24 times, or to a scale of 10 feet to the Moon's diameter. It is proposed to make a photographic map of the entire visible surface of the Moon on that scale, also one on the scale of 3 feet to the Moon's diameter, in each of which every crater will be shown under at least two different illuminations,—under morning and evening Sun, for example.

The superb views from the top of Mount Hamilton are well illustrated by a panorama by Mr. COLTON. Beside these and his photographs of several of the principal constellations, Mr. COLTON has secured some exceptionally fine photographs of the Observatory and surroundings under varying conditions of weather. The views of cloud forms, fog and snow effects are especially attractive to those who live in lower altitudes. Some of Mr. PERRINE's snow pictures, especially those showing the curious deposit on the trees, of frost formed directly from the clouds blowing over the surface of the mountain, remind one of the winter scenes around Niagara Falls.

The exhibit taken as a whole is one of which all may be proud, those who provided the material, as well as all friends of the Observatory and University. That it has been appreciated is shown plainly by the crowds passing before it day after day. If it awakens in only a few of those who pass by an interest in the science of astronomy, or even a desire to add one new fact to their knowledge, it will have done a good work, not to mention

the pleasure and information it has given to those who are enthusiasts already and who have been privileged to study it.

The Harvard College Observatory (Professor E. C. PICKERING, Director), has sent to the Fair an interesting and important exhibit of photographs and engravings which illustrate its long and honorable history, and which show the progress of the great works now being carried on in both hemispheres in photography, photometry and in spectroscopy. The exhibit is well displayed in a large room adjacent to the space occupied by the LICK Observatory.

The Physical Laboratory of the JOHNS HOPKINS University also occupies an adjacent space with an exhibit of the magnificent spectrum maps made by Professor ROWLAND. One of ROWLAND's concave diffraction gratings forms a part of the exhibit and is so displayed as to project the spectrum of the Sun by day and of the electric arc lights by night.

(FIFTEENTH) AWARD OF THE DONOHUE COMET-MEDAL.

The Comet Medal of the Astronomical Society of the Pacific has been awarded to W. F. DENNING, Esq., F. R. A. S., of Bristol, England, for his discovery of an unexpected comet on March 26, 1894.

The Committee on the Comet-Medal,

EDWARD S. HOLDEN,
J. M. SCHAEBERLE,
CHAS. BURCKHALTER.

May 26, 1894.

ON THE FORMS OF THE DISKS OF *JUPITER'S*
SATELLITES.*

The contradictory results obtained by different observers of *Jupiter's* satellites seem to indicate that with our present instrumental means it may be impossible to decide definitely the exact form of celestial bodies presenting very small disks. The evidence

* Reprinted from the *Astronomical Journal*.

seems to prove that the unavoidable errors of judgment in different conscientious observers are often due to radically different causes. Just how these different errors are produced is not clear, but that some of the observers are wrong the discordances in the results prove.

The case of *Jupiter's* first satellite furnishes a good example. In a careful series of observations of this body made with the special purpose of determining the form of the disk, Professor BARNARD, with one single exception, recorded the satellite as circular in outline on eighteen different occasions.† To Professor CAMPBELL and myself the satellite appears round only when it is very near to or projected on the disk of *Jupiter*, and elongated in the direction of the planet's equator in all other positions.‡ Professor HOLDEN likewise satisfied himself during our observations that the disk of I was elliptical and that of III round, when the satellites were off the disk. Professor W. H. PICKERING on the other hand observed regular periodic changes of such a nature that the interval of time between a circular phase and the next following (or preceding) maximum elliptical phase was only about six and one-half hours.§ These last mentioned observations were made at Arequipa and have not been verified elsewhere.

As an illustration of my own observations of Satellite I, made near its western elongation for the special purpose of detecting a rapid change of phase, the results of a series of measures of the major and minor axes of the elliptical disks as drawn by me on December 9, 1893, are here given :

Mount Hamilton P. S. T. of Drawing.		Ratio of the Diameters.
H.	M.	
8	40	0.84
9	35	0.86
11	25	0.83
12	10	0.80
13	20	0.83
14	20	<i>Note:</i> No drawing. (0.83) Same as at 13 ^h 20 ^m .

It will be seen that during a continuous interval of 5^h 40^m, my observations gave a practically constant form for the outline; during this time all the other satellites appeared nearly round.

As bearing directly upon the Arequipa satellite observations,

† *Astronomy and Astro-Physics* for April, 1894, page 272.

‡ *Publications of the A. S. P.*, 1891, Vol. III, page 355.

§ *Astronomy and Astro-Physics* for May, 1893, page 390.

a significant fact, apparently showing the great superiority of the Mount Hamilton results over those obtained by Professor W. H. PICKERING, will now be briefly considered.

In a paper,* published in No. 305 of the *Astronomical Journal*, I proved that the apparently abnormal forms of the shadows which I repeatedly observed here, and which had at diverse times been observed by others, were not abnormal at all but required by theory. The past literature of this subject reveals the fact that some of the ablest astronomers must at times have regarded the shadows as round when, as the theory will prove, they were very far from being so. So strongly has the heretofore generally received idea—that the shadows as viewed from the earth should always be practically circular—influenced the judgment of observers in general, that the comparatively few records describing the true form of the shadow when it was not round, have been regarded with more or less doubt, at times even by the actual observers of the phenomenon.

Now one would naturally suppose than an observer, after having discovered, as he believed, a periodic variation in the form of a satellite, would seek to verify his results by examinations of the satellite's shadow during its transit across the disk of the planet. Taking the January, 1893, observations of Satellite I as observed at Arequipa, I find that the longest (longitudinal) diameter of every shadow just after its ingress on the visible disk of *Jupiter* was more than twice the breadth of the shadow, while at egress just the reverse condition of things existed. That this enormous distortion escaped the scrutiny of the Arequipa observers seems to be shown by the published evidence, for nowhere, so far as I know, does it appear that the repeatedly distorted shadow outlines were ever observed at Arequipa. At the LICK Observatory on the other hand, the systematic recurrences of the various phenomena during the shadow's transit were so persistently observed by myself that they led to the discovery of the true explanation.

J. M. SCHAEBERLE.

LICK OBSERVATORY, June 9, 1894.

* A Simple Geometrical Explanation of the Cause of the Observed Distortions of the Shadows of *Jupiter's* Satellites.

THE LOWELL OBSERVATORY, IN ARIZONA.

BY EDWARD S. HOLDEN.

Mr. PERCIVAL LOWELL has lately established an observatory near Flagstaff, Arizona, at an elevation of 7300 feet above the sea.* According to press telegrams, this station is connected with the Harvard College Observatory (Director, Professor EDWARD PICKERING), and is to be under the charge of his brother, Professor WILLIAM PICKERING of the Harvard Observatory, who has lately been Director of the Harvard station in Arequipa, Peru.†

I have just received a copy of the Boston *Commonwealth* of May 26, 1894, which contains a long account of a paper read by Mr. LOWELL before the Boston Scientific Society on his new observatory and its plans for work. It is to be presumed that this abstract of Mr. LOWELL'S paper is authoritative; but as it is not signed by his name, he may not hold the views there expressed. I have quoted below a few paragraphs from this account (the italics are my own), because they seem to me to be very misleading and unfortunate; and all the more so because they are very well written, and in some respects quite true. They are likely to be widely copied in other periodicals and widely read by many intelligent persons who are interested in the results of astronomical research, but who are not sufficiently instructed in the details of it to form independent judgments. It seems to be the first duty of those who are writing for such a public to be extremely cautious not to mislead; and especially to avoid over-statement. Conjectures should be carefully separated from acquired facts; and the merely possible should not be confused with the probable, still less with the absolutely certain. It

* See *Publications A. S. P.*, Vol. VI, page 122.

† A letter from Mr. JOHN RITCHIE, Secretary of the Boston Scientific Society (received at Mount Hamilton, June 7) notifies the A. S. P. that the press-telegram printed in these *Publications*, Vol. VI, page 122, was incorrect. The telegram sent from Cambridge on February 12 stated that the new observatory in Arizona was to be a station of the Harvard College Observatory, whereas the fact is that the expedition is a private one, sent at the expense of Mr. LOWELL, who pays the salaries of the observers (they being on leaves of absence from Harvard) and who is the chief of the expedition. Mr. RITCHIE'S letter concludes: "I enclose Mr. LOWELL'S statement before the Boston Scientific Society, as published in the *Commonwealth* of Boston;" the enclosure is the article from which extracts are given above.

seems that the writer has not always observed these obvious rules. I have added to the quoted paragraphs a few remarks of my own, which are not intended as a criticism upon the writer of the article, but are primarily designed to point out statements which I think would be misleading to most readers. The quotations follow :

“With regard to the observatory’s plan of work, its main object is the study of our solar system. *This may be put popularly as an investigation into the condition of life in other worlds, including last, but not least, their habitability by beings like if (or ?) unlike man. This is not the chimerical search some may suppose. On the contrary, there is strong reason to believe that we are on the eve of pretty definite discovery in the matter.* In the first place, analogy warrants us in conceiving this little ball on which we dwell in the sea of space as no more the sole vehicle of intelligent life than it was once thought to be the pivot on which the whole cosmic system turned. Just as it is now known to be but one of many bodies revolving around the Sun, so, doubtless, is it but one of many worlds evolving in due course the phenomena of intelligent life. *If the nebular hypothesis be correct, and there is good reason at present for believing in its general truth, then to develop life more or less distantly resembling our own must be the destiny of every member of the solar family* which is not prevented by purely physical considerations, size and so forth, from doing so. In LAPLACE’S day, so much was but the natural deduction from celestial mechanics alone. Since then, however, we have got collateral evidence of another kind, from the astro-physical contributions of modern astronomy.

“The first bit of this *new* evidence may be said to have been the observation of the great red spot on *Jupiter*. Investigation into this led to the discovery that that giant globe still glows by its own inherent heat. Photometric observations of *Saturn* suggested a like state of things there. Finally, the spectrum of *Uranus* shows strange markings that point in the same direction. All of which goes to prove that in the case of the outer planets we are looking on globes midway in their course of development from stars to worlds. In the inner planets we see a very different state of things; a state more nearly that which our own Earth has reached. While in the Moon we gaze upon the last sad age of decrepitude, a world almost *sans* air, *sans* sea, *sans* life, *sans* everything.

"The next phase of the problem opened with SCHIAPARELLI's great discovery, in 1877, of the so-called canals of *Mars*. Scouted at first by less penetrating observers, the existence of such canals has since been amply confirmed. Speculation has been singularly fruitful as to what these markings on our next to nearest neighbor in space may mean. Each astronomer holds a different pet theory on the subject, and pooh-poohs those of all the others. Nevertheless, *the most self-evident explanation from the markings themselves is probably the true one; namely, that in them we are looking upon the result of the work of some sort of intelligent beings*. In short, just as the great red spot on *Jupiter* implies that the planets may become habitable, the amazing blue network on *Mars* hints that one planet besides our own *is actually inhabited now*. What further heightens interest in the matter is that *Mars* has, from other considerations, undoubtedly reached a much later stage in planetary development than our *Earth* has yet attained, and that in gazing upon him we are in a sense peering into futurity.

"*Mars* is the most likely object for results to such planetary investigation. But there is evidence that something in the same general evolutionary line may be learnt from our other relatives in space—*Venus*, for example. From certain observations, it would appear that, belying her name, she is the most modest of all the orbs, keeping herself constantly cloaked in clouds—a state of things corresponding to our own carboniferous age. And we seem to be on the point of learning much more. *The increasing apertures of modern objectives*, taken in connection with what has recently been done and with the revelations of the spectroscope, *show that we stand upon the threshold of a knowledge of our closest of kin in the world of space, of the most important character.*"

The LOWELL Observatory is to be devoted to "an investigation into the conditions of life in other worlds, including their habitability by beings like, or unlike, man." On this sentence I remark that any serious study of any planet by any means is, in its degree, a study of "the conditions of life" on that planet. A measurement of the angles of a crystal of fluorspar in a laboratory is part of a study of "the conditions of life" on the *Earth*. An accurate drawing of the planet *Mars* is, in its way, no doubt, a study of "the conditions of life" on that planet. But I submit that the phrase "the conditions of life"

is misleading. It holds out hopes that are very unlikely to be realized in the year of grace 1894, or during this century. What these hopes are is roughly indicated by the next phrase, "their habitability by beings like, or unlike, man. This is not the chimerical search some may suppose." The foregoing words seem to me to be especially misleading. Does the writer intend to say that there is any probability whatever that either of the two following questions can be settled within the next decade?

First.—Are *Mars*, *Venus*, etc., inhabited?

Second.—Are their inhabitants like (or unlike) human beings?

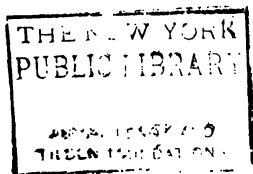
The words I have quoted seem to me to imply that there is a probability that these questions will soon be settled. I think many readers of the *Commonwealth* will so interpret them. But I also think that nearly every living astronomer will agree with me in saying, as I do, that there is no reasonable probability whatever of any such settlement at the present time. The problem is one for the considerably distant future, as has been well pointed out by Professor SEARLE in the *Publications A. S. P.*, Vol. II, page 165 (1890). What is there in the nebular hypothesis to show or even to hint that it "*must* be the destiny of every member of the solar family to develop life?" The nebular hypothesis has nothing whatever to say on the question of the *origin* of life; or on the evolution of organic beings of any kind. Its problem is—*given* a fiery mist containing all the chemical elements, and *given* plenty of time; *to account* for the present constitution and conditions of the inorganic constituents of the solar system. LAPLACE never dreamed of accounting for the beginning of organic life. DARWIN's book treats of the origin (*i. e.*, the differentiation) of *species*; not of the origin of *life*. SPENCER gives no authoritative account of the commencement of life. Sir WILLIAM THOMSON is obliged to bring organic life to the Earth on a meteorite in order to invent a beginning for organic evolution. Was not the meteorite very hot when it came to the Earth? It was certainly very cold in the stellar spaces. And where did this first organic cell originate, at any rate? Where did its "life" come from?

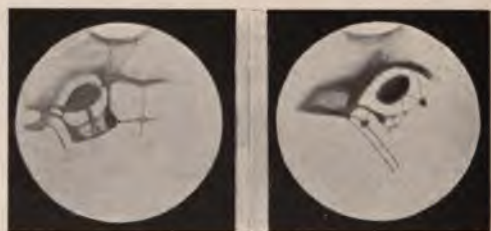
These questions, inexpressibly important as they are, have nothing to do with the nebular hypothesis, either in its old or new state. And the "new" evidence spoken of by the writer: how new is it? Most of it would be new to LAPLACE, no doubt.

But I think the average reader would derive the idea that he was hearing about very recent discoveries. Of course, the writer of the article knew how old some of these facts are; but I submit that he has, unintentionally no doubt, misled his audience. It *may* be that "the most self-evident explanation from the markings (on *Mars*) is probably the true one; namely, that in them we are looking upon the result of the work of some sort of intelligent beings." It just may possibly be true, though by no means "self-evident;" but I, for one, declare that I cannot find a scintilla of evidence in favor of that view. The writer is speaking of the so-called *canals* on *Mars*; these are long, dark narrow markings on a reddish background, and they are sometimes many hundreds, even thousands, of miles long, and seldom less than 50 miles wide. Sometimes they are visible, and sometimes they are not; they appear and disappear; sometimes they are double and sometimes single. Does the foregoing sound like a description of *canals*? If it were, should we think these waterways were built by intelligent beings like ourselves, or by madmen? Will not a canal 50 miles wide accommodate the shipping on *Mars*? And if more water surface is required, why not widen the canal? Why dig another one parallel to it? And, in the name of sane engineering, why fill it up every now and again only to open it the next season?

Again, how does the writer know that the dark markings on *Mars* are water and the red markings land? It may be so, but has it been proved? For my part, I only know a couple of arguments in favor of this hypothesis, and they are not conclusive. But the writer of the quoted paragraph assumes this to begin with, and I maintain that in doing so he is stating a probability as if it were a certainty; and he is stating it so well and easily as to mislead his readers. Finally, is it true that the large objectives of modern telescopes are especially well fitted to deal with faint contrasts like those on the planet *Venus*, so as to hold out a reasonable hope of new discoveries "of the most important character" regarding this planet? I doubt it, speaking for myself; and I can give at least one good reason for my conclusion.

With these few remarks, which could be carried much further, I leave the paragraphs in question. They seem to me to hold out hopes that are not likely to be quickly realized, and therefore to be suited to mislead rather than to lead the reader. They are all the more persuasive for being well written. But





August 14, 11^h 15^m.

August 17, 11^h 15^m.

DRAWINGS OF *MARS*, 1892.

By Professors CAMPBELL (left-hand) and HUSSEY (right-hand); Lick Observatory.

they tell only half-truths; and the particular half-truths that the whole human race would be delighted to have verified. It is the business of observatories—Mr. LOWELL's and others—to labor to obtain new light on such fundamental questions. It is the duty of all instructed observers to be scrupulously exact in announcing results for a popular audience. It seems to me that the writer I have quoted has not been sufficiently careful in this respect.

The very essence of the scientific habit of mind is conscientious caution; and this is especially necessary in referring to matters in which the whole intelligent world is interested—as the condition of the planet *Mars*, for example. I may take as an example the telegrams regarding *Mars* sent by cable from South America in 1892 by Professor WILLIAM PICKERING, who is to be the chief observer at the Lowell Observatory in Arizona. I quote two out of many such telegrams:

“NEW YORK, October 6, 1892.—The *Herald* correspondent at Valparaiso cables as follows: Professor PICKERING of the Harvard Branch Observatory at Arequipa says that he discovered forty small lakes in *Mars*.”

How does he know the dark markings are lakes? Why does he not simply call them dark spots? And is he sure there are forty?

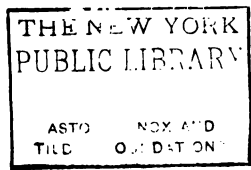
“NEW YORK, September 2, 1892.—Professor PICKERING of Harvard College sends the following to the *Herald* from Arequipa, Peru:

“*Mars* has two mountain ranges near the south pole. Melted snow has collected between them before flowing northward. In the equatorial mountain range, to the north of the gray regions, snow fell on the two summits on August 5 and melted on August 7. I have seen eleven lakes near *Solis Lacus* varying in area from 80 by 100 miles to 40 by 40 miles. Branching dark lines connect them with two dark areas like seas, but not blue. There has been much trouble, since snow melted, in the Arean clouds. These clouds are not white, but yellowish and partly transparent. They now seem to be breaking up, but they hang densely on the south side of the mountain range. The northern green spot has been photographed. Many of SCHIAPARELLI's canals have been seen single.”*

*Several of these canals were seen not only single but double at Mount Hamilton. I do not know that they were so seen in Peru.

How is it known that there are two polar mountain ranges? How does he know that the flow will be northwards? And an equatorial range? Are not the gray regions so extensive that the description is, to say the least, indefinite? What is the evidence of "trouble" in the clouds? Is it certain that no clouds on *Mars* are white? How about the clouds "twenty miles high" reported by Professor PICKERING? Were they not white?

These and similar telegrams from South America regarding the happenings on *Mars* in the year 1892 were received by the astronomers at the LICK Observatory with a kind of amazement. We were observing *Mars* at that time (and continuously during the whole of each night) with a 36-inch telescope under good conditions. We have considerably more than a hundred drawings of the planet. Very many of Professor PICKERING'S conclusions could not be derived from our LICK Observatory drawings (which contained far more detail than his, however). Could they be justified by those he was making in Peru with a 13-inch telescope? This latter question could not possibly be settled in North America till we, also, saw the drawings on which Professor PICKERING'S conclusions were founded, and accordingly we waited. When the Peruvian drawings were finally published (*Astronomy and Astro-Physics*, 1892, page 668), it was at once obvious to all experts that they were not a sufficient proof of the reported discoveries. That our members may make comparisons for themselves, I insert some illustrations—first, a plate giving all of Professor PICKERING'S drawings which have been published; second, plates showing some Mount Hamilton drawings by Professors CAMPBELL and HUSSEY. We have over a hundred such, as I have said. Some of them have already been printed in the *Publications A. S. P.*, Vol. V, page 131, *et seq.*, and I refer to these also; as well as to others yet to appear. Good drawings are sometimes reproduced by engravers in a poor way. I have myself suffered from their neglect. But these drawings of Professor PICKERING'S are very well reproduced. It is obvious, at first glance, that the superstructure of announcement is too elaborate for the base of observations (so far as drawings like these represent the observations). All experts will agree that such diagrams as those made in Peru, unaccompanied by full verbal notes and measures, are not convincing evidence of important announcements. They are still (June, 1894) the only published



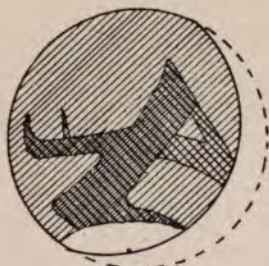


FIG. 1.

May 9, 21^h 05^m



FIG. 2.

July 14, 16^h 50^m



FIG. 3.

July 16, 17^h 45^m



FIG. 4.

July 17, 15^h 50^m



FIG. 5.

July 23, 17^h 30^m



FIG. 6.

July 25, 20^h 40^m

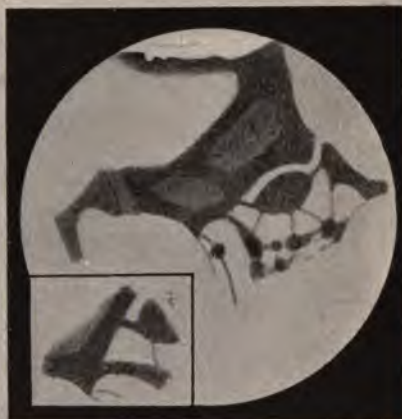
MARS; MAY-JULY, 1892.

By Professor WILLIAM PICKERING at Arequipa, Peru.



W. J. Hussey

1892 JULY 23^d 12^h 50^m - P. L. C.
 $\lambda = 74^\circ$ $\beta = 14^\circ$



W. J. Hussey

1892 AUGUST 20^d 12^h 50^m
 $\lambda = 74^\circ$ $\beta = 12^\circ$



Lacaille 1781

AUGUST 20^d 12^h 50^m - P. L. C., 1892.

W. J. Hussey

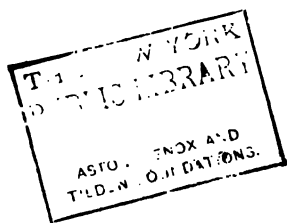


W. J. Hussey

1892 JULY 24^d 11^h 12^m
 $\lambda = 82^\circ$ $\beta = 14^\circ$

DRAWINGS OF MARS, 1892.

By Professor Hussey; Lick Observatory.



evidence available regarding observations made two years ago, in July, 1892. Some of Professor PICKERING's results do not agree with the conclusions so far reached at Mount Hamilton. For that reason, we are justified in doubting them until convincing evidence is at hand, as it certainly is not now.

Professor PICKERING's observations of clouds "twenty miles high" on *Mars* are discussed by Professor CAMPBELL in *Publications A. S. P.*, Vol. VI, page 108 (1894).

I may quote another telegram from Peru:

"NEW YORK, October 14.—The *Herald* received the following cable from Arequipa, Peru, to-day: '*Jupiter's* fourth satellite is very dark now. The third satellite is girdled by a very dark belt in the northern hemisphere, inclined 20 degrees to the orbit. It can be best seen during the transit. The second satellite is probably not spherical. The first satellite is egg-shaped, and revolves end over end, and nearly in the orbital plane. Its period is twelve hours and fifty-five minutes. PICKERING.'"

In other places Professor PICKERING has given details regarding his observations of the behavior of the satellites of *Jupiter*.

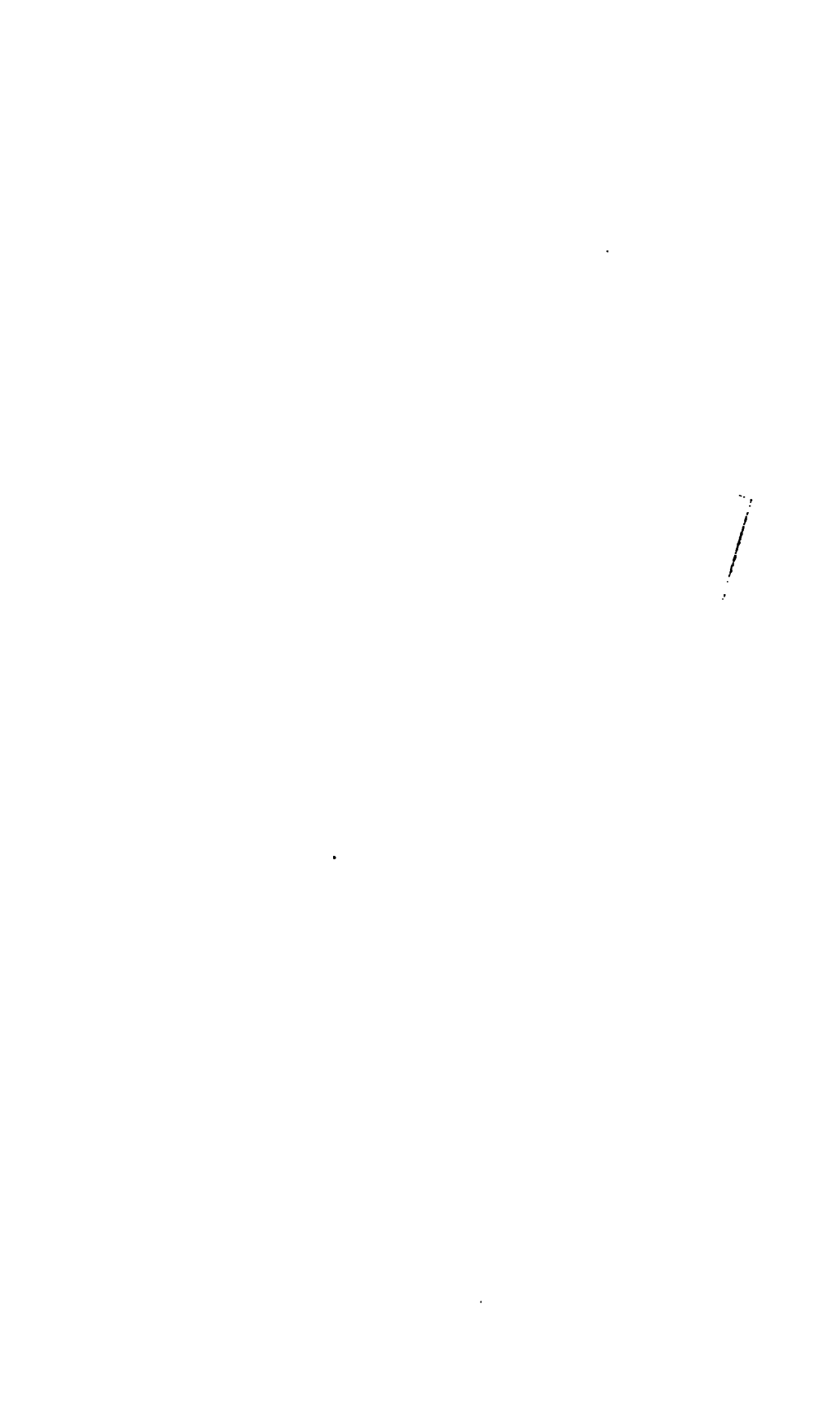
Professor PICKERING's conclusions as to the first satellite of *Jupiter* may (possibly) be true; but continued observations at the LICK Observatory, for some years past, do not lead us to accept them. Professor PICKERING's observations on *Jupiter* were made in 1892. They have not been verified by anyone during the very favorable opposition of 1893. I can see no reason why any one of a dozen large telescopes, now active in the northern hemisphere, should fail to see in 1893 what was seen with a 13-inch in Peru in 1892. The observations of Professors SCHAEBERLE, BARNARD, CAMPBELL, and my own, at the LICK Observatory distinctly contradict those made in Peru. In No. 3229 of the *Astronomische Nachrichten* (which was received after this paper was written) is a note from Professor PICKERING regarding the bright belt on *Jupiter's* first satellite which Professor BARNARD described in the *Monthly Notices*, R. A. S., for January, 1894, and which was observed by him during 1893. Professor PICKERING says that this bright belt is not "a permanent one, for it certainly did not exist at the time of the opposition of 1892." By this he can only mean that he did not see it in Peru with the 13-inch equatorial, and that so far as his observations go, it did not exist. If he had turned to the *Publications* of the A. S. P., for 1891, pages 355-357, he would have seen that Professors

SCHAEBERLE and CAMPBELL regularly observed the belt during September and October, 1891. Its existence was fully demonstrated at this time, which is the reason why there are no further published observations by these gentlemen. Do not the published observations of this belt at the LICK Observatory during 1891 and 1893 indicate that the Arequipa observations of 1892, which fail to show the belt, are, in so far, erroneous and incomplete? If this bright belt, which is obvious in our 36-inch telescope, was not visible in the 13-inch telescope at Arequipa, is it not likely that most of the differences between the Mount Hamilton and Arequipa observations are to be laid to insufficient optical power at the latter station? Such a solution is the most natural one to adopt, at least until adequate proof is brought forward (as it has not yet been) that this supposition is incorrect.

In this connection I refer to a paper by Professor SCHAEBERLE, printed in this number of the *Publications* A. S. P., and to his remarks on the shape of the shadows of the satellites of *Jupiter*. They are seen, at certain times, to be twice as long as they are broad, and this phenomenon is regularly observed here. I have myself photographed this appearance. I believe it was not noticed in Peru, which is strange, considering how much more difficult observations are reported.

At the coming opposition of *Mars* (1894) Professor PICKERING will have the use of an 18-inch telescope. An 18-inch is considerably more powerful than a 13-inch, but it is considerably less so than our 36-inch. It is to be hoped that the observations to be made in 1894, at the Flagstaff and Mount Hamilton Observatories, will agree better than those made in 1892, at Arequipa and Mount Hamilton. If they do not, it cannot be considered unreasonable on our part to maintain that our work at the LICK Observatory is deserving of the greater weight (since we have the better opportunity), at least until convincing proof is brought forward to the contrary. Such proof we shall be the first to accept. Evidence of the kind which is now available in support of the Arequipa observations of 1892 will not be sufficient.

Finally, I wish to disclaim, in the frankest manner, any desire to uselessly criticise the paper in the Boston *Commonwealth*, or the observations of Professor PICKERING in Peru. I have felt it imperative to remark on what seems to me an unfortunate and misleading tendency in the article referred to; and also to point out that the Peruvian observations of *Mars* and *Jupiter* (which



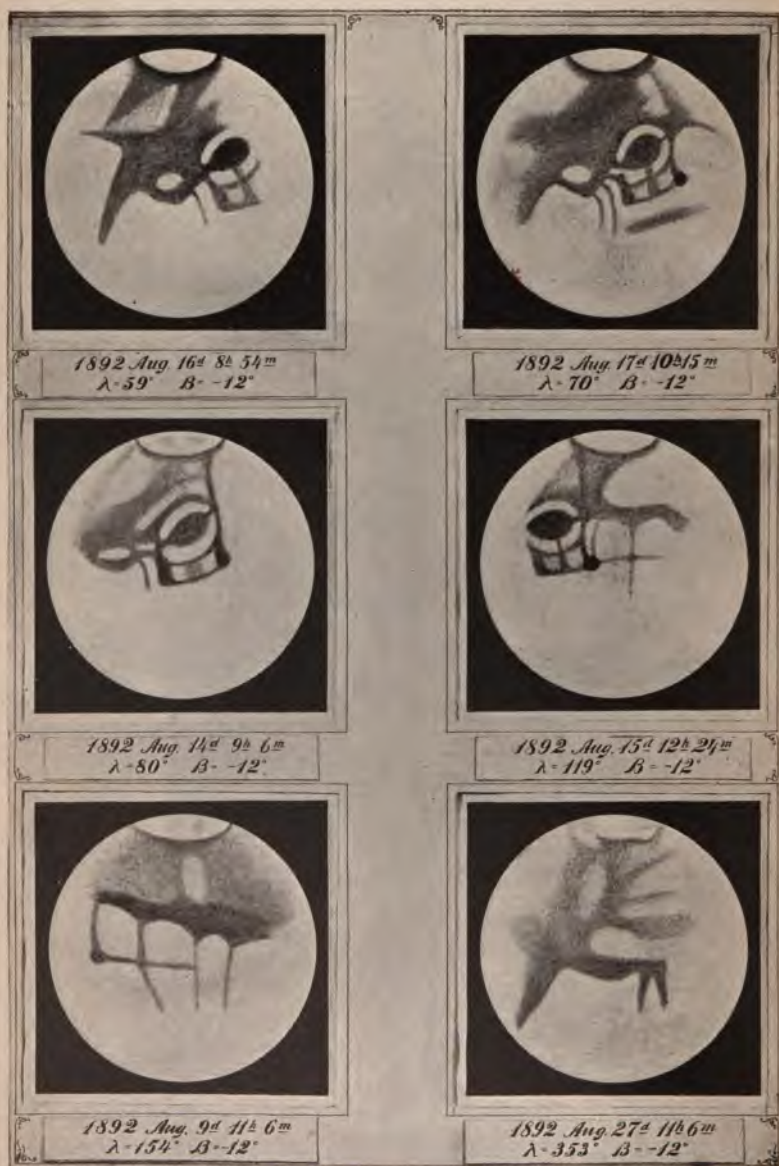


PLATE C¹.

MARS, 1892.

By Professor W. W. CAMPBELL; LICK Observatory.

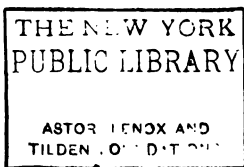
might seem to support the conclusions of the article) are themselves in serious need of confirmation. I trust I have been able to do this in all courtesy; and at the same time in a manner which will carry conviction.

EDWARD S. HOLDEN.

THE LICK OBSERVATORY, June 1, 1894.

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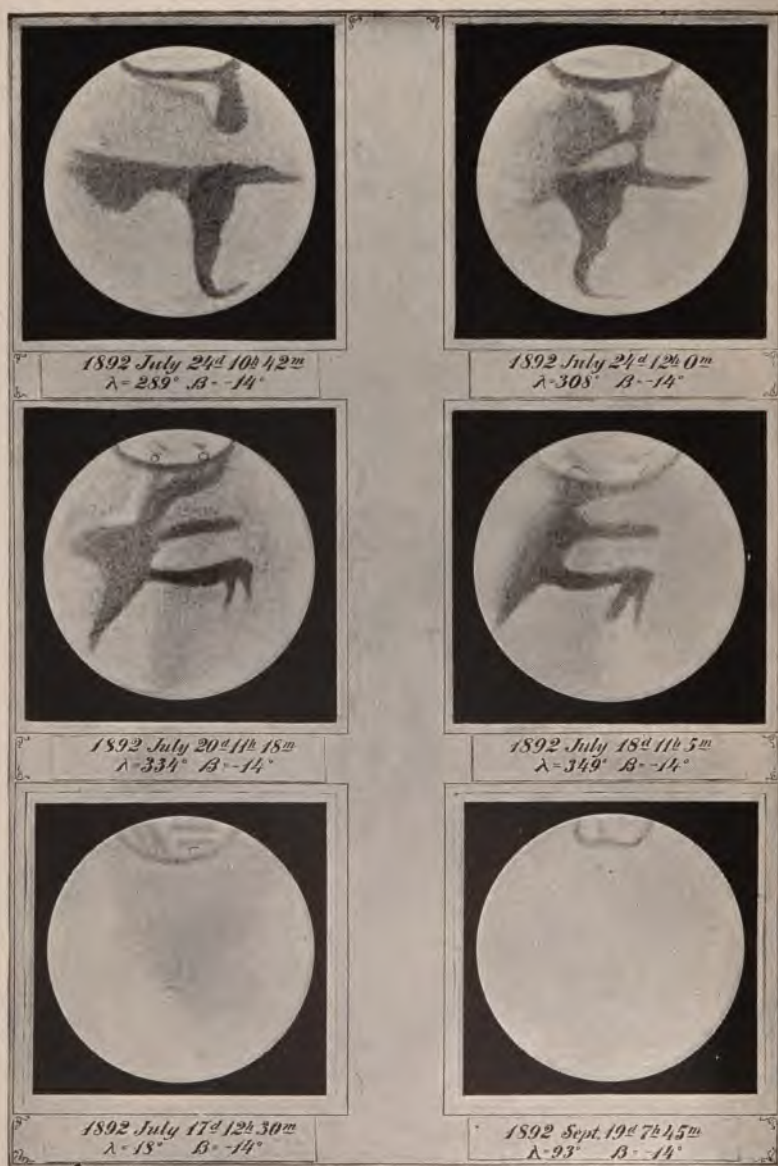


PLATE C².

MARS, 1892.

By Professor W. W. CAMPBELL; LICK Observatory.



NOTICES FROM THE LICK OBSERVATORY.

PREPARED BY MEMBERS OF THE STAFF.

DRAWINGS OF THE PLANET *MARS*.

The two plates of drawings of *Mars*, C¹ and C², are reproduced as accurately as possible from twelve original sketches made in 1892. My observations of the planet were confined to parts of four nights per week, between July 10 and August 17, with the exception of parts of two later evenings. Earlier observations were prevented by absence from the Observatory, and later ones by work on the spectrum of the new star in *Auriga*. On many of the nights the atmosphere was very unsteady, and prevented observations. The unfavorable nights, combined with the three nights per week missed by me, account for the fact that my drawings do not cover some parts of the planet's surface. The region in the vicinity of *Lacus Solis* was observed on several nights under favorable circumstances, and the drawings relating to that portion are more satisfactory than any of the others. Several especially bright spots on the polar cap and elsewhere are represented by enclosing them by bounding lines. No description of the drawings is given here, for the reason that the LICK Observatory intends to print the observations of *Mars*, made and to be made during 1892 and 1894, in a separate volume. The drawings hitherto and now published serve as a sample of the material already on hand.

Mr. C. D. PERRINE assisted me very materially in transferring the sketches from the note-books to the finished drawings.

W. W. C.

MOUNT HAMILTON, June 7, 1894.

TEMPERATURE ON THE SURFACE OF FIXED STARS AND OF THE
SUN, COMPARED WITH THAT OF TERRESTRIAL HEAT-
SOURCES,* BY PROF. J. SCHEINER, POTSDAM.

Investigating the spectra of the brighter stars by means of photographs, made at the Potsdam Observatory, I observed the peculiar behavior of a line which belongs to the spectrum of magnesium ($448.{}^{\text{mm}}$). This line is very prominent, on account of its breadth or intensity, in nearly all spectra of Class I; it even reaches the breadth of the hydrogen lines in spectra of this class which are poor in lines. It is also very prominent in the spectra of *Sirius*, *Vega*, *Procyon*, and others (which are richer in lines), although not so much so as in the last mentioned case. In the solar spectrum, and in other spectra of the Class IIa, the line is weak, and could not be found at all in some stars belonging to this class, and it seems that it becomes weaker the more nearly the star-spectrum approaches Class III. In the artificial spectrum of magnesium the intensity and breadth of this line is also subject to great changes. The line is not observed in the spectrum of free-burning magnesium, and of magnesium vapor in the electric arc; but it reaches great intensity and breadth in the electric-spark spectrum. This had already been found by LIVEING and DEWAR,† and the investigations of KAYSER and RUNGE, as well as my own, have confirmed the correctness of their observations.

At first sight we might be induced to explain the peculiarity of the line by a difference in temperature of magnesium vapor in the electric arc and the electric spark, and draw conclusions regarding the temperatures on the fixed stars. Since, however, it is impossible to distinguish sharply between the influence of temperature and pressure, we are only permitted to conclude that the magnesium vapor on stars of Class I is in a condition similar to that produced by the electric spark of great intensity; and on stars of Class II in a condition similar to that produced by the electric arc.

Another magnesium line ($435.2{}^{\text{mm}}$), however, shows, according to my observations, an entirely opposite behavior to the one discussed above. It does not appear in any of the spectra of those stars of Class IIa which are poor in lines, but becomes

* Translated by Mr. C. A. STETEFELDT, for the Society.

† *Proced. R. Soc.*, Vol. XXX.

visible in the spectra of those which are rich in lines; it is very prominent in the Sun and in some stars of Class IIa, and appears in the spectrum of α Orionis (Class IIIa) as one of the strongest lines. In the laboratory this line also shows an opposite behavior to that noted for 448.2^{mm}. It is hardly or not at all visible in the spark spectrum, but very strong and broad in the electric arc spectrum. LIVEING and DEWAR had already noticed the peculiarity of this line.

The favorable circumstance that two lines belonging to the same substance show opposite behavior, is a proof that the phenomena which these lines present on the stars can only be a function of temperature and not of pressure. Increased pressure broadens all lines of a gas and makes them more prominent. It cannot happen, according to KIRCHHOFF's law, that a line becomes smaller with increased pressure. On the contrary, it is a well known fact that single lines may become weaker and smaller at higher temperature, while, generally speaking, the lines become stronger and broader under such conditions. Hence, I feel justified in announcing the following :

The temperature of the so-called absorbing strata—the upper strata of the photosphere—on the stars, Class IIIa, is approximately equal to that of the electric arc (about 3000° to 4000° C.); it is higher on the Sun and the stars of Class IIa, but does not reach the intensity of the spark of a Leyden jar; on the stars of Class Ia it approximates the temperature of this spark (upper limit about 15,000° C.).

This result furnishes at the same time the first direct proof for the correctness of the physical meaning of VOGEL's spectral classification, according to which Class II has been developed by cooling of Class I, and Class III by still further cooling of Class II.

LETTER FROM DR. VEEDER REGARDING THE AURORA OF
FEBRUARY 22, 1894.

“ PROFESSOR E. S. HOLDEN, LICK Observatory, Mount
Hamilton, Cal.

“ *Dear Sir* : You will perhaps be interested to learn that the aurora of February 22 last, which was seen at LICK Observatory and elsewhere in Southern California, New Mexico, Arizona, etc., was not generally seen eastward in the United States until the

following day, when it became conspicuous along the Atlantic coast. Careful inquiry develops the fact that the telegraph lines were disturbed by earth currents westward from Chicago exclusively on February 22; such disturbance not extending eastward until the day following, when the aurora did likewise. This localization of the aurora in longitude is most interesting, and is being actively investigated in connection with the system of concerted observations of the aurora undertaken in co-operation with the expedition of Lieutenant PEARY. The officials of the Western Union Telegraph Company have been interested, and more complete memoranda of the distribution of earth currents on their lines are being made.

"The relations to characteristic solar conditions which I have pointed out in notes and articles heretofore, are being verified most completely. Yours truly, M. A. VEEDER."

LYONS, N. Y., April 30, 1894.

ROTATION OF THE PLANET SATURN.

"The latest important results on this question are due to Mr. STANLEY WILLIAMS,* who has taken every pains for the determination of an accurate value, and to free the results from any possibility of their being influenced by preconceived ideas. With regard to the method of observation employed, and the details of the observations themselves, we must refer the reader to the publication mentioned [in the foot note], but a brief summary of the results may not be out of place.

"The observations were made in 1893, and two kinds of spots were observed: (1) dark spots upon a conspicuous double belt in the northern hemisphere; and (2) bright spots in the equatorial zone.

"In the case of the former, the period was obtained from numerous spots, but eleven of them have been used as giving well-ascertained values, a table of these figures showing that they can be arranged into two classes, the means of which are $10^h 14^m 29^s.07$ and $10^h 15^m 0^s.74$. Between these values there is a difference of over half a minute, a quantity too large, judging from the way sets of observations agree *inter se*, to be due to errors of observations.

"With the bright spots a similar result is noticed, only here

* *Monthly Notices of R. A. Society*, Vol. LIV, No. 5, March, 1894, page 297.

the difference is not the same. Out of the five series of deduced values four may be coupled well together, giving a mean value of $10^h 12^m 59^s.36$. The fifth or outstanding value is 13 seconds shorter than this.

“These different values for the periods of rotation point out pretty distinctly that the spots that have been observed are by no means fixed relatively to the planet’s surface, but are endowed with a proper motion of their own. In the case of the dark spots the surface material must have rotated over half a minute more quickly in the same latitude upon one side of *Saturn* than upon the other. Mr. STANLEY WILLIAMS summarizes the results of his discussion in the following words:

“Between north kronocentric latitudes 17° and 37° the surface material of *Saturn* rotated in 1893 at the rate of $10^h 14^m 29^s.07 \pm 0^s.27$ between longitudes 45° and 140° , and at the rate of $10^h 15^m 0^s.74 \pm 0^s.56$ between longitudes 175° and 340° ; whilst between longitudes 340° and 45° there was a region in which the surface material rotated at a rate intermediate between the above values.

“Between north kronocentric latitude 6° and about 2° south latitude, the surface material of *Saturn* rotated in 1893 at the rate of $10^h 27^m 59^s.36 \pm 0^s.27$ between longitudes 0° and 140° , whilst between longitudes 140° and 360° the rate of rotation was rather quicker, the average period of rotation here being well represented by $10^h 12^m 45^s.8$.”

“The importance of such results as those stated above will help considerably to unravel the mystery surrounding the circulation of the envelope of this great planet; but, for the observations to be made comparable, they must be accurate, systematically made, and extend continuously over moderately long periods. For the years 1891–93 Mr. STANLEY WILLIAMS points out that the acceleration in the motion of the bright equatorial spots can be clearly deduced from the different periods of rotation.

“They are for—

	h	m	s
1891	10	14	21.8
1892	10	13	38.4
1893	10	12	59.4”

—Extract from a paper by W. J. L., in *Nature*, May 10, 1894.

ORBIT OF COMET *A*, 1894.

"DENNING'S COMET.—M. L. SCHULHOF (*Astronomische Nachrichten*, 3227) has computed an elliptic orbit for the comet found by Mr. DENNING on March 26, as the parabolic elements previously determined did not satisfy the observations. The period of the comet appears to be 6.745 years. According to the criterion published by M. TISSERAND some time ago, the comet is identical with either GRISCHOWS' comet (1743 I) or BLANPAIN'S (1819 IV), or it may be with both, for the identity of these two objects is admitted by some astronomers. M. SCHULHOF points out that it is desirable that DENNING'S comet, which is fading rapidly, should be followed so long as possible with large telescopes. Periodic comets can only throw light upon some obscure points in celestial mechanics and cosmogony when they have been observed during several apparitions."—From *Nature*, May 10, 1894.

STUDENTS AT THE LICK OBSERVATORY, 1894.

The following students are taking work at the LICK Observatory during the summer of 1894:

Professor ROBERT GRANT AITKEN, B. A., M. A. . . . Williams College.
(Now Professor of Mathematics in the University of the Pacific.)

Miss MARY EUNICE EDWARDS, B. A. Stanford University.

Mr. JOHN SIMEON HODGIN, B. S. Pacific University, Oregon.

Mr. ARTHUR FRENCH POOLE Stanford University.

VISITORS TO THE LICK OBSERVATORY, 1888-1894.

The visitors' books of the Observatory show:

6400	visitors	admitted	for the year	ending	June 1,	1889
5132	"	"	"	"	"	1, 1890
5005	"	"	"	"	"	1, 1891
5959	"	"	"	"	"	1, 1892
5472	"	"	"	"	"	1, 1893
5747	"	"	"	"	"	1, 1894
33,715	"	"	"	"	years	1888-1894.

About ten per cent. of the visitors do not register.

E. S. H.

RESEARCHES UPON COMET V, 1889, BY DR. CHARLES
LANE POOR.

NOTE: In the JOHNS HOPKINS University circular for May, 1894, Dr. POOR has an extremely interesting abstract of his researches upon the orbit of BROOKS' comet (1889, V) discovered on July 6, 1889. An abstract, by Professor CAMPBELL, of some of Dr. POOR's work has already been printed in these *Publications* (Vol. V, 1893, page 232), to which the reader is referred. What follows is quoted directly from Dr. POOR's paper, with a few omissions: E. S. H.

“AS TO IDENTITY OF BROOKS' COMET WITH COMET
LEXELL, 1770.

“LEXELL'S comet underwent its notable disturbance in the year 1779, and, moreover, this disturbance took place in that part of *Jupiter's* orbit in which Comet V, 1889, suffered its great change of elements, as above discussed, in the year 1886. Between these two appulses there intervened a period of 107 years, which period must be accurately accounted for in order to establish the identity of these two remarkable bodies. But, assuming the substantial correctness of the present investigation, we cannot directly account for these necessary years. For the period of Comet V in 1884, or previous to its disturbance, has been shown to be 31.38 ± 1.50 years, which is not an aliquot part of 107. Hence, unless in the intervening years the comet suffered other and marked disturbances in its orbit, the entire question as to the identity between the two bodies falls at once. A further investigation shows us that such disturbances did take place during this interval, but leaves us utterly in the dark as to the resulting changes in the orbit. The uncertainty in the original observations become so magnified in this part of the comet's orbit that we can no longer trace its path with absolute accuracy, we cannot say with certainty that the two comets are or are not identical. The probability now seems to be that they are not one and the same body. For a definite solution of the problem we must await the re-appearance of the comet in 1896. The first few observations in that year will furnish * * * a final answer to the question of identity.

“These changes in the orbit of the comet will be better under-

stood by referring to Figure 1. The three concentric circles represent respectively the orbits of the Earth, *Jupiter* and *Saturn*; the small ellipse, part full, part dotted, the present orbit of the comet, while the two large ellipses, with the shaded space between them, the possible orbits of the comet before the great disturbance by *Jupiter*, in 1886. All these various paths for the comet are tangent to one another at the point of closest approach to the orbit of *Jupiter*, namely, *P*. This figure shows at a glance the great change that took place in the motion of the body.

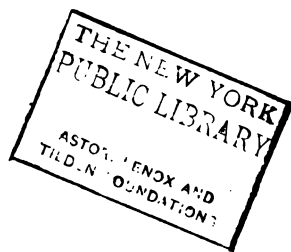
"The heavily drawn portion of the small ellipse represents that part of the comet's path which it was actually seen to describe during the years 1889 and 1890. The remainder of this curve and the large ellipses were determined by tracing the comet backward step by step: such curves are entirely theoretical. The still outstanding uncertainty in the present orbit of the comet is so slight that it cannot be represented in the diagram. But unfortunately we cannot speak so definitely in regard to the orbit before 1884; the uncertainty is here much magnified and is clearly shown in the diagram.

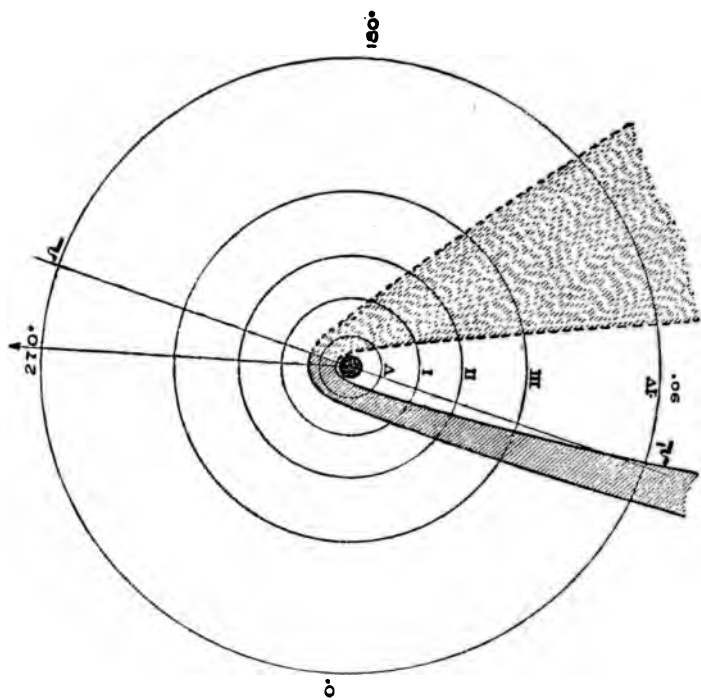
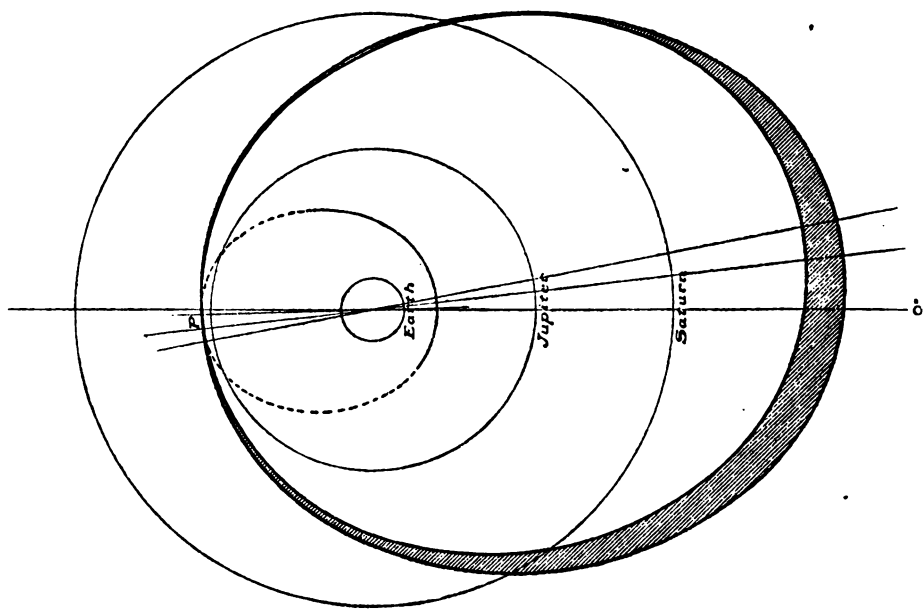
"All that we can definitely say at present is that previous to 1884 the comet was traveling about the Sun, in an ellipse of about 31 years' period; that its path lay somewhere within the shaded portion of the figure; that during nearly a century this path was undisturbed. In whatever one of these various paths the comet was traveling it reached the point of tangency in the summer of 1886; at this same time *Jupiter* arrived at the corresponding part of its orbit, and the comet was gradually drawn from the large to the smaller ellipse. In this smaller ellipse it will continue to travel for a number of years, returning to perihelion in the first part of November, 1896. It should be first seen by the aid of the LICK telescope in May, 1895, and with medium sized instruments in the spring of 1896."

THE PASSAGE OF COMET V, 1889, THROUGH JUPITER'S SATELLITE SYSTEM, BY DR. CHARLES LANE POOR.

[Abstract of papers in *Astronomical Journal*, Nos. 302 and 309.]

"In 1886, *as above stated*, the comet passed very close to the planet *Jupiter*; probably within the orbit of the fifth satellite. Exactly how close the two bodies were at any given time is impossible to say; the uncertainty introduced through the original





observations here becomes very prominent and precludes many interesting investigations. We can, however, readily place limits within which the comet certainly passed. It may be stated quite definitely that the comet passed the center of the planet at a distance not greater than 3.65 and not less than 1.00 radius of the planet itself. In other words, the center of the comet may have grazed the surface of *Jupiter*, and it certainly approached that surface to within 2.63 radii of the planet, or only 112,300 miles.

"In Figure 2 I have tried to represent what actually occurred. The five satellites of *Jupiter* revolve about that planet in ellipses differing but slightly from circles, and all lying nearly in the same plane. In Figure 2 we are supposed to be looking down upon this plane and to see these satellite orbits in their full size. The orbit of the comet was in a plane inclined nearly 70° to this, and intersecting it in the line Ω, Ω' . The projection of the comet's path on the plane of the satellites' orbit is given, and, as the actual path is unknown, I have drawn two curves between which the true orbit is certainly known to lie. The most probable path of the comet is a curve about midway between the two. It must be understood that during the time the comet was thus moving about *Jupiter* that body was in rapid motion itself in the direction of the large arrow, and it carried with it the entire system, satellites and comet.

"A careful inspection of the figure will show that the comet rose up suddenly from below the plane of the satellites' orbits, crossing it nearly at right angles, then passed upward and almost directly over *Jupiter*, and then gradually descended and finally passed below this orbital plane again. So that, as the comet neared *Jupiter*, there could be no close approach to any of the satellites, excepting at the point where it passed through the plane of their motion. But, on the other hand, as the comet receded from the planet, it hovered over the satellites, and close approaches might occur, provided that the satellites were in the proper places at the right time.

"A careful investigation of all possible positions of the comet and of the satellites show that a collision was impossible; that the comet did not even approach near enough to any one of the four large satellites to cause the slightest change in the relative motion of the nucleus about *Jupiter*. Such a collision, or, at least, a close appulse, has been repeatedly invoked, to account for the observed

disruption of the comet. (The comet, as observed, consisted of four separate parts.) * * * *

"There are many interesting problems connecting this comet with the fifth satellite of *Jupiter*. There is certainly great probability that the satellite passed directly through the comet; the mean path of the comet intersected that of the satellite, so that a direct collision was possible. Unfortunately the slight uncertainty in the comet's motion is here complicated by an additional uncertainty in the motion of the satellite, which renders it impossible to say definitely that a collision did or did not take place. It is possible, therefore, that the observed disruption of the comet was caused by the action of the satellite. But, all things considered, it is more probable, I think, that it was caused by *Jupiter* itself.

"*Jupiter* differs greatly from a sphere; its ellipticity being one-seventeenth. Hence, in treating of its action upon the comet, the shape of the planet must be taken into account. An investigation shows that the introduction of terms depending upon the shape of the planet caused little or no difference in the actual motion of the nucleus about the Sun. On the other hand, we find in the unequal attractions, due to the non-sphericity of *Jupiter*, on the various parts of the comet, when at perijove, a ready explanation of the observed disruption.

"Among the many interesting problems yet to be fully discussed is this entire question of the breaking up of the comet; as well as the possibility that a portion of it was permanently drawn into the Jovian system, thus forming a new satellite or satellite ring."

THE VARIATION OF TERRESTRIAL LATITUDES, BY DR. S. C. CHANDLER.

"At the meeting of the Boston Scientific Society, on March 27, Dr. S. C. CHANDLER presented a report of progress of his investigation of the Variation of Latitude. During the past six months he has been engaged in the reduction of POND'S observations made more than half a century ago at Greenwich, England. In this work he has been assisted by Miss WENTWORTH and Miss COBB, graduates of Boston University, whose skill in the discussion of the problems before them is worthy of much praise."

"The axis of the earth which we imagine to connect the

poles, as represented in the diagrams in the geographies, is termed by astronomers the 'axis of figure,' and is the shortest diameter of the flattened sphere of the Earth. Some years ago it became evident from anomalies in astronomical observations that some phenomenon was producing a curious change in the latitude of all places on the Earth's surface. A consideration of the problem by Dr. CHANDLER convinced him that the Earth did not revolve about its axis of figure, but about another axis which was constantly shifting its position. This other axis describes a circle at the poles, of about 22 feet radius, the time necessary for the completion of its change being 427 days. Closer figuring on the observations soon showed that a secondary revolution of this axis was also in progress. This second revolution is a motion also circular in its nature, in a smaller circle of only 5 feet radius, which is accomplished in a year almost exactly. The periods of these two motions are not the same, and for that reason sometimes they are pulling together, in which case the resultant motion is their sum; and at other times they do not pull in the same direction, and then the amount of the disturbance is diminished. The combined effect of the two motions results therefore in a spiral, with a maximum radius of about 28 feet, which in three and one-half years is reduced to only 2 or 3 feet, and in a like time becomes again as great as before.

"Although the fact that these motions exist has now been accepted almost without exception by astronomers, there has been but little success in the determination of their causes. In order to determine as exactly as possible the laws of the different motions, Dr. CHANDLER has considered, one after the other, all the principal series of observations which have had for their object the determination of latitude. There have been many of these series, and he has been at work for nearly two years, with assistants for the larger part of the time, re-reducing the observations and determining their agreement with his laws. Among other series, he has taken up those of POND, and it is especially of the results of this work that he desired to speak.

* * * * *

"The POND series has the advantage over others because it covers a sufficient length of time to show the complete round of the combined motions of the two polar movements the period of which, as has been said, is some seven years. These observations furnish also a stepping-stone to those of BRADLEY of nearly

a century before, a time too great to bridge accurately from recent observations, but easily reached now by means of the POND observations. This additional large number of years, by means of which the number of periods of complete variation of latitude is very much increased, furnishes a rigid test of the accuracy of Dr. CHANDLER's computations. From this he finds that his first period, deduced from a comparatively small number of observations, was only a day in error, and that the true time of the revolution of the pole in the larger circle is very nearly 428.6 days.

"It has been shown that the relations of the two motions of the pole are such that sometimes the resulting motion is a large one and sometimes it is but small. At the present time we have just entered a period of small variation. This is a crucial test, and the observations of the next six or eight months will either prove or disprove the accuracy of Dr. CHANDLER's laws. Already statements published by the observatories of Pulkowa and Strasburg, results given to the world within a few weeks, show that at both these places the observations confirm the law.

"One or two related points were briefly commented upon by the speaker. The variation in latitude should affect also to some extent the pointing of meridian circle instruments, and a recent investigation of this matter undertaken by SOKOLOFF has determined that this variation has a period of 428 days, agreeing remarkably well with the variations in latitude.

"Another interesting point is that the French astronomers have been among the very last to allow that any such motion as the variation exists. At last they have prepared an instrument and are about to take some observations. It is curious that during the immediate future, the time during which they will be likely to work, the variation will be very small, and it would not be surprising if they should consider this to be further evidence that the motion does not exist.

"Another result of the investigations has been the elimination of what has been known as temperature derangements. These errors, which have been ascribed to differences in temperature, have been exceedingly annoying, but they now seem to be mostly referable to the known variations of the pole, and for that reason a higher precision and greater refinement in the observations becomes possible."—From the *Boston Commonwealth*, March 31, 1894.

NUMBER OF CLEAR DAYS AND TOTAL RAINFALL IN DIFFERENT PARTS OF THE MONTHS OF MAY AND JUNE, AT THE LICK OBSERVATORY, IN THE YEARS 1881 TO 1894.

The cloudy weather which just ended on the night of June 6 was remarkable, being continuously unfavorable for observing for eight days, with rain every day.

There is no record in previous years of so few clear days at this season, as will be seen by the following comparison, which is based upon the meteorological classification into clear, fair and cloudy, a clear day being one on which the cloudiness of three observations at 7 A. M., 2 P. M., and 9 P. M., is less than 10 on a scale in which 30 would be entire cloudiness for all three observations.

	NUMBER OF CLEAR DAYS.				RAINFALL.	
	MAY 1-10.	MAY 11-20.	MAY 21-31.	JUNE 1-6.	MAY.	JUNE 1-6.
1881..	7	9	8	5	.09	
1882..	9	9	8	4	.48	
1883..	1	3	10	6	7.55	
1884..	10	6	6	4	1.24	
1885..	8	5	11	6	0.16	
1889..	3	5	11	5	3.21	
1890..	5	10	8	6	2.42	
1891..	7	7	5	5	1.01	0.13
1892..	0	9	11	6	3.52	
1893..	5	6	11	6	.95	
1894..	10	2	4	0	2.78	0.43

C. D. P.

LICK OBSERVATORY, June 7, 1894.

THE LOWE OBSERVATORY, ECHO MOUNTAIN, LOS ANGELES COUNTY, CAL.; DR. LEWIS SWIFT, DIRECTOR.

During the year 1893 financial difficulties made it necessary for Mr. WARNER, the patron and founder of the WARNER Observatory of Rochester, N. Y., to withdraw his support from that institution, in which Dr. LEWIS SWIFT had done so much work. The instruments of the observatory belonged to Dr. SWIFT himself, and for some time it was expected that arrange-

ments might be made to install them at the State University of Colorado (Boulder City). In the early part of the present year (1894) it was decided by Dr. SWIFT to accept a proposition made to him by Professor T. C. LOWE of Pasadena, Cal., and to move his instruments—including the 16-inch CLARK Equatorial formerly at Rochester—to Echo Mountain, near Pasadena, a site some 3500 feet above the sea, and some 2 miles distant from WILSON's Peak, where the Harvard College Observatory formerly maintained an observatory station. It is understood that Dr. SWIFT's present station is at the terminus of Professor LOWE's electric railway, not far from a fine hotel. The establishment of this new observatory is an important event in the astronomical history of California.

E. S. H.

SLIGHT EARTHQUAKE SHOCK, MAY 7, 1894.

On Monday, May 7, I had the unusual experience of detecting a movement of the Earth by the meridian circle, a movement which was too slight, probably, to be recorded by any seismograph, however delicate. This was followed, at an interval of an hour and five minutes, by an actual earthquake shock on the mountain. Whether the first can be considered as the premonitory signal of a movement of greater force to follow, or whether it was simply the outer edge of a shock which occurred at some distance, with no necessary connection with our actual local occurrence later on, can be left to future discussion. The chance of catching these slight movements is not great.

The nadir observation, for the level of the meridian circle, was in progress, and had to be suspended while the movement lasted. The instrument was apparently swaying regularly and with a slow, even swing in the east and west direction, the only motion noticeable.

It must have been the mountain in oscillation, a movement not sensible, probably, by any other means. Mounted on a high step-ladder, I could detect nothing of it myself.

This movement lasted for 15 to 20 seconds, and was of several seconds of arc in extent. The unexpectedness of the occurrence made it puzzling at the time, and I was more concerned to see that nothing was wrong with the instrument, than in making such measures as might have given more exact data had I known the meaning of the movement. The clock face was not illumi-

nated, so that the time is only given to the nearest minute : 10^h 52^m P. M., Pacific standard time.

As the level observation had been partially made previous to the commencement of the movement, I was able to assure myself that no change had occurred in the position of the instrument.

One hour and five minutes later came an actual shock, sensible to others in the Observatory as well as to myself, and which had the effect of disturbing the instrument in zenith distance by 3". The amount of change, however, has no relation to the intensity of the shock. The circle had been read in part after the observation of a star, and by returning over the same microscopes the effect could be readily seen.

This appeared to me as one single, sharp shock. The approximate time was 11^h 56^m 45^s, Pacific standard time, though no attempt was made to record this exactly. R. H. T.

THE ORBIT OF 70 OPHIUCHI.

This interesting binary star has recently been thoroughly investigated by Dr. SCHUR, and his results have been given in a late number of the *Astronomische Nachrichten*, No. 3220. It is an interesting object historically, since it has been under observation since double stars were first measured, the list of observers including every name of note in this department of research. It also affords one of the best objects for the computation of an orbit, especially since the companion has already completed a revolution about the primary star since accurate measures were begun.

It was one of the earliest stars measured by Sir WILLIAM HERSCHEL, 1779, when he had still in view the possible determination of stellar parallax by means of such measures of double stars; that is, of stars optically double, lying close together in the line of sight, but without physical connection.

The elder STRUVE began upon this star in 1819 and continued to observe it till 1837. Sir JOHN HERSCHEL and SOUTH measured it during this period. BESSEL, with the Königsburg heliometer; ENCKE, GALLE and others at Berlin, MÄDLER at Dorpat, KAISER at Leiden, and OTTO STRUVE at Poulkova, are famous names. Coming down to later times, Baron DEMBOWSKI at Vienna, Father SECCHI at Rome, and WINNECKE at Berlin and Bonn, ENGLEMAN, DUNÉR, SCHIAPARELLI and

SCHUR are among later Continental observers. In England, DAWES, DOBERCK, SMYTHE, GLEDHILL, WILSON and SEABROKE; while in this country Professor MITCHELL at Cincinnati was among the early contributors, followed by BOND, WINLOCK and SEARLE at Harvard College, HALL and FRISBY at Washington; and among later observers BURNHAM, STONE, WILSON and COMSTOCK.

The orbit of this binary has been several times computed, the period having ranged from 94 to 80 years. It was the subject of Dr. SCHUR's thesis for his degree, as long ago as 1868.

Since a complete revolution has been made the period is now known with great exactness, the definite result from Dr. SCHUR's discussion being 88.4 years, the only point of general interest in the elements of the orbit. The latest measures included in this discussion are those made by himself, as yet unpublished, and the set observed at the LICK Observatory in July of last year, printed in No. 31 of these *Publications*.

Upon all the observations available the orbit of the binary is based, and the original observations are then compared with an ephemeris computed from the elements of the orbit. When more than one series of measures has been made in a year by the same observer these are combined into a single result; and in this manner there are finally 333 yearly sets of measures to be compared with the ephemeris place.

Among later observers, the difference between the computed and measured place will occasionally reach 3° in position angle and $0''.5$ in distance. An error of 3° in the position angle corresponds to an error of $0''.1$ in terms of the distance between the stars. Professor HALL gives for his probable error of a single measure at distance $3''$, in position angle $\pm 0''.065$ and in distance $\pm 0''.08$. But in comparing the observations with the definite orbit, the systematic errors, which each observer will have undoubtedly to some degree, will appear with full evidence. STRUVE and a few other observers of extensive experience have made determinations of their personal equation for the different classes of double stars. SCHUR has collected, in the discussion of 70 *Ophiuchi*, the results of each observer when four or more yearly sets had been made, and these illustrate well the existence of personal equation in the measurement of double stars, and the need of taking every precaution to as far as possible eliminate it from the results. A reversing prism is recommended as used by many thorough and practiced observers.

The residuals of the series made at Mount Hamilton from the computed place, were $0''.00$ in distance and $0^{\circ}.54$ in position angle.

The question of a definite difference between the results given by heliometer and micrometer in the measurement of small distances as illustrated in the history of this star, remains still an open one. BESSEL appeared to obtain constantly a larger distance with the heliometer, than STRUVE with the micrometer, at the same epoch.

As to the future of this double star, the apparent distance will decrease after this year until, in 1902, it will be a minimum: $1''.6$. And it will be below $2''$ until 1906.

The measurement of the position angles and distances of three small stars in the neighborhood of *70 Ophiuchi* affords a good illustration of the possibility of determining the proper motion of the principal star from such material.

The only other measures of these stars that I know of were made by Professor HALL; the latest in 1886. Plotting the two sets of measures, the accordance between the direction and amount of proper motion, as given by this means, and by meridian circle observations of the principal star, is quite good. R. H. T.

THE PARALLAX OF *70 OPHIUCHI*.

The results of a new determination of the parallax of this star, by measuring its position with reference to two other stars at distance of $15'$ and $29'$ respectively, have been published by Dr. SCHUR in the *Astronomische Nachrichten*, No. 3231.

Professor KRUEGER used the same stars at Bonn, 1858 to 1862, and found for the parallax of *70 Ophiuchi* $\pi = + 0''.150$. By his request Dr. SCHUR redetermined the value in 1891 and obtained $+ 0''.286$. These are not in satisfactory agreement; but adopting a parallax of $0''.2$, with the major axis of the ellipse of the orbit of the companion of *70 Ophiuchi* $4''.6$, and the period 88.4 years, the mass of the system would be 1.6 times the Sun's mass. The companion would be distant from the principal star 23 times our distance from the Sun; *70 Ophiuchi* would be 1,000,000 times as far from the Earth as the Sun is, and its light would require 16 years to reach us.

The stars used in the above discussion were not the ones measured by Professor HALL, and at the LICK Observatory; these last were very faint stars and much nearer *70 Ophiuchi*, the distances ranging from $56''$ to $162''$, all being visible in the $5'$ field of the 36-inch telescope, power 350. R. H. T.

MINUTES OF THE MEETING OF THE BOARD OF DIRECTORS,
HELD AT THE LICK OBSERVATORY, JUNE 9, 1894.

Professor CAMPBELL took the chair; and a quorum was present. The minutes of the last meeting were approved. The following members were elected:

LIST OF MEMBERS ELECTED JUNE 9, 1894.

Dr. HENRY H. BATES	The Portland, Washington, D. C.
Mr. HUGH CRAIG	312 California Street, S. F., Cal.
Mr. WILLIAM T. DAWSON	Black Bear, Siskiyou Co., Cal.
Mr. GEORGE C. HOLBERTON	15 First Street, S. F., Cal.
Mr. L. S. J. LALLIE, C. E.	1615 Arapahoe St., Denver, Colo.
Mr. PERCIVAL LOWELL	{ Lowell Observatory, Flagstaff, Arizona.
CALIFORNIA STATE LIBRARY	Sacramento, Cal.
Dr. LEWIS F. MIZZI	Constantinople, Turkey.
General W. J. PALMER	32 Nassau St., New York, N. Y.
Mr. ALEXANDER RYDZEWSKI	{ Officer Street, House 13, Lodg- ing 26, St. Petersburg, Russia.
Mr. FRANCIS SINCLAIR	Berkeley, Cal.
Dr. A. BLAIR THAW	{ San Ysidro Rancho, Santa Bar- bara, Cal.

It was

Resolved, To print in the next *Publication* of the Society a detailed statement of expenditures prepared by Mr. ZIEL.

It was also agreed that the following resolution be voted on at the September meeting:

Resolved, That one-half of any credit balance remaining in the General Fund at the date of each Annual Meeting shall be transferred to the Life Membership Fund, until said fund contains \$50 for each person whose name has been borne on the roll of life members since the date of organization of the Society.

Adjourned.

STATEMENT OF EXPENDITURES FROM THE GENERAL FUND, 1889-1894.

MARCH.	PUBLICATIONS.		Other Printing and Stationery.	POSTAGE.			Express.	Furniture.	Rent.	Sundries.	TOTAL EXPENDI- TURES.	TOTAL RECEIPTS.
	Printing.	Illustrat'ns.		Lick Obs.	San Fran.	Total.						
1889-90....	\$ 418 70	\$108 00	\$135 15	\$ 95 00	\$ 42 95	\$137 95	\$13 15	\$ 46 50	\$ 35 00	\$130 80	\$1,025 25	\$2,147 00
1890-91....	1,128 93	191 89	185 52	205 00	76 45	281 45	84 06	282 79	75 00	118 25	2,347 89	1,980 63
1891-92....	1,128 15	152 57	181 82	192 74	92 00	284 74	57 89	36 30	260 00	389 16	2,490 63	3,387 17
1892-93....	999 85	207 75	198 37	176 00	130 00	306 00	32 02		240 00	410 32	2,394 31	2,437 22
1893-94 .	817 90	82 25	162 20	190 00	53 70	243 70	21 12	31 00	240 00	404 36	2,602 53	2,137 59
TOTALS....	\$4,493 53	\$742 46	\$863 06	\$858 74	\$395 10	\$1,253 84	\$208 24	\$396 59	\$850 00	\$1,452 89	\$10,260 61	\$12,089 61
Illust'ns	\$742 46								\$ 378 39			
	\$5,235 99									1,450 61	\$1,839 00	
											\$12,089 61	\$12,089 61

Balance in General Fund.
Balance in Life Members' Fund.

(E. & O. E.)

F. R. ZIEL,

Treasurer.

SAN FRANCISCO, APRIL 1, 1894.

MINUTES OF THE MEETING OF THE ASTRONOMICAL SOCIETY
OF THE PACIFIC, HELD AT THE LICK OBSERVATORY,
JUNE 9, 1894.

Professor CAMPBELL presided. The minutes of the last meeting as printed in the *Publications*, were approved.

The Secretary read the names of members duly elected at the meeting of the Directors.

The following papers were presented:

1. The Candelaria Meteor of February 1, 1894, by Professor H. A. NEWTON of Yale College.
2. Vassar College Observatory, by the Director, Professor M. W. WHITNEY.
3. Astronomical Photographs at the Midwinter Fair, by Mr. A. H. BABCOCK of San Francisco.
4. Theory of the Modern Spectroscope, illustrated by the new MILLS Photographic Spectroscope of the LICK Observatory, by Professor CAMPBELL.
5. The Spectrum of Comet GALE, by Professor CAMPBELL.
6. Sun-spot Photographs at the LICK Observatory, by Mr. C. D. PERRINE.
7. The LOWELL Observatory of Arizona, by Professor E. S. HOLDEN.
8. Proper Motion of 70 *Ophiuchi*, by Mr. R. H. TUCKER, JR.

Professor HOLDEN read a paper upon the LOWELL Observatory of Arizona.

Mr. TUCKER presented and explained a diagram showing the proper motion of 70 *Ophiuchi*, as derived from small stars observed by Professor HALL and himself.

Professor CAMPBELL explained the new MILLS photographic spectroscope, and illustrated the spectrum of the GALE Comet.

Adjourned.

OFFICERS OF THE SOCIETY.

W. W. CAMPBELL (Lick Observatory), ... *President*
 W. J. HUSSEY (Leland Stanford Jr. University, Palo Alto, Cal.), }
 WM. M. PIERSON (Mills Building, S. F.), } *Vice-Presidents*
 JOHN DOLBEER (10 California Street, S. F.), }
 C. D. PERRINE (Lick Observatory), ... *Secretary*
 F. R. ZIEL (410 California Street, S. F.), ... *Secretary and Treasurer*
Board of Directors—Messrs. ALVORD, CAMPBELL, DOLBEER, HOLDEN, HUSSEY, MOLERA,
 PERRINE, PIERSON, SCHAEFERLE, VON GELDERN, ZIEL.
Finance Committee—Messrs. PIERSON, DOLBEER, VON GELDERN.
Committee on Publication—Messrs. HOLDEN, CAMPBELL, YALE.
Library Committee—Messrs. VON GELDERN, MOLERA, BABCOCK.
Committee on the Comet-Medal—Messrs. HOLDEN (*ex-officio*), SCHAEFERLE, BURCKHALTER.

OFFICERS OF THE CHICAGO SECTION.

Executive Committee—RUTHVEN W. PIKE.

OFFICERS OF THE MEXICAN SECTION.

Executive Committee—CAMILO GONZALEZ, FRANCISCO RODRIGUEZ REV, AGUSTIN ARAGON.

NOTICE.

The attention of new members is called to Article VIII of the By-Laws, which provides that the annual subscription, paid on election, covers the *calendar* year only. Subsequent annual payments are due on January 1st of each succeeding calendar year. This rule is necessary in order to make our book-keeping as simple as possible. Dues sent by mail should be directed to Astronomical Society of the Pacific 819 Market Street, San Francisco.

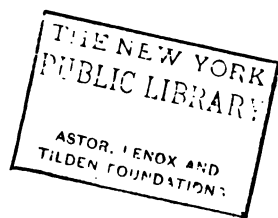
It is intended that each member of the Society shall receive a copy of each one of the *Publications* for the year in which he was elected to membership and for all subsequent years. If there have been (unfortunately) any omissions in this matter, it is requested that the Secretaries be at once notified, in order that the missing numbers may be supplied. Members are requested to preserve the copies of the *Publications* of the Society as sent to them. Once each year a title-page and contents of the preceding numbers will also be sent to the members, who can then bind the numbers together into a volume. Complete volumes for past years will also be supplied, to members only, so far as the stock in hand is sufficient, on the payment of two dollars to either of the Secretaries. Any non-resident member within the United States can obtain books from the Society's library by sending his library card with ten cents in stamps to the Secretary A. S. P., 819 Market Street, San Francisco, who will return the book and the card.

The Committee on Publication desires to say that the order in which papers are printed in the *Publications* is decided simply by convenience. In a general way, those papers are printed first which are earliest accepted for publication. It is not possible to send proof sheets of papers to be printed to authors whose residence is not within the United States. The responsibility for the views expressed in the papers printed rests with the writers, and is not assumed by the Society itself.

The titles of papers for reading should be communicated to either of the Secretaries as early as possible, as well as any changes in addresses. The Secretary in San Francisco will send to any member of the Society suitable stationery stamped with the seal of the Society, at cost price, as follows: a block of letter paper, 40 cents; of note paper, 25 cents; a package of envelopes, 25 cents. These prices include postage, and should be remitted by money-order or in U. S. postage stamps. The sendings are at the risk of the member.

Those members who propose to attend any or all of the meetings at Mount Hamilton during the summer should communicate with "The Secretary Astronomical Society of the Pacific" at the rooms of the Society, 819 Market Street, San Francisco, in order that arrangements may be made for transportation, lodging, etc.







EYE-END OF THE 36-INCH TELESCOPE.

(By the courtesy of *McClure's Magazine*.)

PUBLICATIONS
OF THE
Astronomical Society of the Pacific.

VOL. VI. SAN FRANCISCO, CALIFORNIA, AUGUST 15, 1894. No. 37.

PLANETARY PHENOMENA FOR NOVEMBER AND
DECEMBER, 1894.

BY PROFESSOR MALCOLM McNEILL.

The following brief notes on the Sun, Moon and planets, have been prepared at the request of the Council, and are designed for the aid of those interested in astronomy and who do not have an almanac. In the descriptive paragraphs Pacific Standard time is given, unless the contrary is indicated.

NOVEMBER.

Mercury is an evening star at the beginning of the month, too near the Sun to be seen. It passes inferior conjunction, and transits across the Sun's disc, on the morning of November 10. The time of conjunction is 10^h 34^m A. M. The transit is an interesting phenomenon to watch, but is not of much scientific value. The planet now becomes a morning star and moves rapidly out to west elongation, which it reaches on November 27. It then rises nearly an hour and three-quarters before the Sun, and will be easily visible for perhaps two weeks. On November 12 *Mercury*, *Venus* and *Uranus* are all close together, but all are too near the Sun to be seen.

Venus is a morning star quite close to the Sun. It rises less than an hour before on November 1. It rapidly approaches the Sun, and comes to inferior conjunction on the morning of November 30.

Mars is in excellent position for observation, as it is above the horizon nearly the entire night. It is in the constellation *Pisces*,

and retrogrades (moves westward) slowly until November 22, when it begins to move eastward. It goes back on a line somewhat to the north of the line on which it travelled westward. By the end of the month its distance from the Earth will be about 57,000,000 miles, forty per cent. greater than it was at opposition, and its light will be only about half as great.

Jupiter is also in good position for observation, being above the horizon most of the night. It is retrograding in the constellation *Gemini*, moving westward a little more than 2° . At the end of the month it is about the Moon's diameter north and east of the 3d magnitude star μ *Geminorum*.

Saturn is a morning star, not in a very good position for observation. At the end of the month it rises at about 4^h A. M.

Uranus comes to conjunction with the Sun on November 7, and is too near the Sun to be seen throughout the month.

Neptune is above the horizon most of the night. It is so faint (only 8th magnitude) that it cannot be seen without a telescope, and it cannot easily be found without the use of setting circles.

DECEMBER.

The Sun crosses the equator and winter begins December 21, 12^h 9^m P. M.

The Moon will pass over the *Pleiades* group, in the constellation *Taurus*, on the night of December 10, and from all parts of the United States a number of occultations may be noted. As the Moon is less than two days from full, most of the phenomena will be so near the bright limb as to be less effective than if they were observed near new Moon, but the phenomenon is well worth observing.

Mercury is a morning star. It rises about an hour and forty minutes before the Sun on December 1, and may easily be seen in the morning twilight. During the month it approaches the Sun, and by the end of the month has nearly reached superior conjunction.

Venus, having passed superior conjunction on November 30, is an evening star, but remains too near the Sun to be seen. At the end of December it sets about half an hour after the Sun.

Mars is still in good view until quite late at night. By the end of the month it sets at about 2^h A. M. During the month it

moves through the eastern part of the constellation *Pisces* into *Aries* about 7° or 8° eastward, and at the end of the month its distance from the Earth is more than twice as great as it was at opposition, and its light less than one-fourth as great. Its distance from us will then be a little more than 80,000,000 miles, only a little less than the distance of the Earth from the Sun.

Jupiter is also in good position for observation, being above the horizon throughout nearly the whole night. It is in opposition on December 22. It is in the constellation *Gemini*, and during the month moves about 4° westward. Its great northern Declination makes this opposition an especially favorable one for observation, as it approaches the zenith within a few degrees at transit. The distance of the planet from us at this opposition is greater than it has been at the two or three preceding. At the opposition of 1892, which occurred soon after perihelion passage, the planet was nearer to us by about 18,000,000 miles than it will be at the opposition of this year. That, however, means a variation of only about five per cent. in distance and ten per cent. in light. Its Declination was then about 17° less, and its position, therefore, much less favorable.

Saturn is a morning star, and by the end of the month rises at about 2^h A. M. It moves eastward a little over 2° in the eastern part of the constellation *Virgo*. The ratio of major to minor axis of the rings is about three to one.

Uranus is also a morning star, rising a little more than an hour later than *Saturn*. It moves about 1° eastward in the constellation *Libra*.

Neptune is in opposition with the Sun on December 6. It is above the horizon throughout most of the night, and is in the eastern part of the constellation *Taurus*.

EXPLANATION OF THE TABLES.

The phases of the Moon are given in Greenwich Mean time, which is 8 hours fast of Pacific Standard time. To find the corresponding Pacific Standard time, subtract 8 hours from that given in the table. In the tables for Sun and planets, the second and third columns give the Right Ascension and Declination for Greenwich noon. The fifth column gives the local mean time for transit over the Greenwich meridian. To find the local mean time of transit for any other meridian, the time given in the table

must be corrected by adding or subtracting the change per day, multiplied by the fraction whose numerator is the longitude from Greenwich, and whose denominator is 24. This correction is seldom much more than 1^m. To find the standard time for the phenomenon, correct the local mean time by *adding* the difference between standard and local time if the place is west of the standard meridian, and *subtracting* if east. The same rules apply to the fourth and sixth columns, which give the local mean times of rising and setting for the meridian of Greenwich. They are roughly computed for Lat. 40°, with the noon Declination and time of meridian transit, and are intended as only a rough guide. They may be in error by a minute or two for the given latitude, and for latitudes differing much from 40° they may be several minutes out.

PHASES OF THE MOON, G. M. T.

First Quarter,	November 5,	3 ^h 16 ^m	P. M.
Full Moon,	November 13,	7 49	A. M.
Last Quarter,	November 20,	2 8	A. M.
New Moon,	November 27,	8 54	A. M.

THE SUN.

1894.	R. A.	Declination.	Rises.	Transits.	Sets.
	H. M.	° ' "	H. M.	H. M.	H. M.
Nov. 1.	14 27	— 14 32	6 31 A.M.	11 44 A.M.	4 57 P.M.
11.	15 7	— 17 31	6 42	11 44	4 46
21.	15 48	— 20 0	6 55	11 46	4 37
Dec. 1.	16 31	— 21 52	7 4	11 49	4 34

MERCURY.

Nov. 1.	15 36	— 21 53	8 8 A.M.	12 53 P.M.	5 38 P.M.
11.	15 0	— 16 44	6 34	11 38 A.M.	4 42
21.	14 38	— 12 53	5 17	10 36	3 55
Dec. 1.	15 12	— 15 42	5 22	10 30	3 38

VENUS.

Nov. 1.	14 0	— 11 1	5 51 A.M.	11 17 A.M.	4 43 P.M.
11.	14 49	— 15 19	6 15	11 26	4 37
21.	15 39	— 18 58	6 40	11 37	4 34
Dec. 1.	16 32	— 21 45	7 5	11 50	4 35

MARS.

Nov. I.	I 31	+ 7 59	4 15 P.M.	10 46 P.M.	5 17 A.M.
II.	I 23	+ 7 50	3 27	9 58	4 29
2I.	I 19	+ 8 6	2 45	9 16	3 47
Dec. I.	I 21	+ 8 45	2 5	8 38	3 11

JUPITER.

Nov. I.	6 27	+ 22 59	8 18 P.M.	3 45 A.M.	11 12 A.M.
II.	6 25	+ 23 1	7 37	3 4	10 31
2I.	6 22	+ 23 4	6 54	2 22	9 50
Dec. I.	6 18	+ 23 7	6 10	1 38	9 6

SATURN.

Nov. I.	13 53	— 9 9	5 37 A.M.	11 9 A.M.	4 41 P.M.
II.	13 57	— 9 33	5 04	10 35	4 6
2I.	14 1	— 9 56	4 31	10 0	3 29
Dec. I.	14 6	— 10 18	3 57	9 25	2 53

URANUS.

Nov. I.	14 52	— 16 5	7 1 A.M.	12 8 P.M.	5 15 P.M.
II.	14 54	— 16 16	6 25	11 32	4 39
2I.	14 56	— 16 27	5 49	10 55	4 1
Dec. I.	14 59	— 16 37	5 12	10 18	3 24

NEPTUNE.

Nov. I.	4 57	+ 21 8	6 57 P.M.	2 16 A.M.	9 35 A.M.
II.	4 56	+ 21 5	6 16	1 35	8 54
2I.	4 55	+ 21 4	5 36	12 55	8 14
Dec. I.	4 54	+ 21 2	4 56	12 14	7 32

PHASES OF THE MOON, G. M. T.

First Quarter,	December 5,	12 ^h 15 ^m P. M.
Full Moon,	December 12,	7 46 P. M.
Last Quarter,	December 19,	11 16 A. M.
New Moon,	December 27,	2 20 A. M.

THE SUN.

1894.	R. A. H. M.	Declination. ° ' "	Rises. H. M.	Transits. H. M.	Sets. H. M.
Dec. I.	16 31	— 21 51	7 4 A.M.	11 49 A.M.	4 34 P.M.
II.	17 14	— 23 2	7 14	11 54	4 34
2I.	17 59	— 23 27	7 20	11 58	4 36
3I.	18 43	— 23 5	7 24	12 3 P.M.	4 42

MERCURY.

Dec. 1.	15 12	— 15 42	5 22 A.M.	10 30 A.M.	3 38 P.M.
11.	16 7	— 19 59	5 55	10 46	3 37
21.	17 11	— 23 16	6 31	11 10	3 49
31.	18 18	— 24 42	7 6	11 39	4 12

VENUS.

Dec. 1.	16 32	— 21 45	7 5 A.M.	11 50 A.M.	4 35 P.M.
11.	17 26	— 23 28	7 27	12 5 P.M.	4 43
21.	18 21	— 24 0	7 44	12 20	4 56
31.	19 15	— 23 18	7 57	12 36	5 15

MARS.

Dec. 1.	1 21	+ 8 45	2 5 P.M.	8 38 P.M.	3 11 A.M.
11.	1 27	+ 9 45	1 28	8 5	2 42
21.	1 37	+ 11 1	12 54	7 36	2 18
31.	1 50	+ 12 29	12 22	7 10	1 58

JUPITER.

Dec. 1.	6 18	+ 23 7	6 10 P.M.	1 38 A.M.	9 6 A.M.
11.	6 12	+ 23 10	5 26	12 54	8 20
21.	6 6	+ 23 13	4 40	12 8	7 36
31.	6 1	+ 23 15	3 51	11 19 P.M.	6 47

SATURN.

Dec. 1.	14 6	— 10 18	3 57 A.M.	9 25 A.M.	2 53 P.M.
11.	14 9	— 10 37	3 21	8 49	2 17
21.	14 13	— 10 54	2 47	8 13	1 39
31.	14 16	— 11 8	2 12	7 37	1 2

URANUS.

Dec. 1.	14 59	— 16 37	5 12 A.M.	10 18 A.M.	3 24 P.M.
11.	15 1	— 16 47	4 35	9 41	2 47
21.	15 3	— 16 56	3 58	9 3	2 8
31.	15 5	— 17 4	3 22	8 26	1 30

NEPTUNE.

Dec. 1.	4 54	+ 21 2	4 56 P.M.	12 14 A.M.	7 32 A.M.
11.	4 52	+ 21 0	4 12	11 30 P.M.	6 48
21.	4 51	+ 20 59	3 31	10 49	6 7
31.	4 50	+ 20 57	2 41	10 9	5 27

PHOTOGRAPHIC OBSERVATIONS OF *ALGOL*.

BY SIDNEY D. TOWNLEY.

During the fall of 1892, while at the LICK Observatory, I made a series of observations to test the value of photographic methods for the determination of the changes in variable stars. After experimenting for a couple of months with various lenses, I obtained the use of the CROCKER photographic telescope, which has a WILLARD lens of 5.9 inches aperture and 30.7 inches focal length. This is one of the instruments employed by Professor SCHAEBERLE in the determination of his formulæ for photographic magnitudes and atmospheric absorption of photographic rays, published in *Contributions from the Lick Observatory*, No. 3.

The size of an image of a star on a photographic plate, for any particular telescope and any particular plate, will be a function of the time of exposure and the brightness (photographic) of the star. Conversely, the size of the image of any particular star, for any given time of exposure, will depend upon the constants of the instrument, such as aperture, focal length, etc., and the "quickness" of the photographic plate.

It is then possible to determine, from experiments, for any particular telescope and plate, an empirical formula which shall express the photographic brightness of a star as a function of the time of exposure and diameter of the image. Professor SCHAEBERLE has made such an investigation as this, and published, in the contribution named above, a table in which the brightness or photographic magnitude is given as a function of the arguments, time of exposure, and the measured diameter of the image.

As a starting point for magnitudes, Professor SCHAEBERLE assumed the photographic magnitude of *Polaris*, at its mean zenith distance at Mount Hamilton, to be 2.00. This was merely provisional, however, for at that time the effect of atmospheric absorption was not known. By his investigation of this subject, Professor SCHAEBERLE found the atmospheric absorption of the photographic rays of *Polaris*, at its mean zenith distance at Mount Hamilton, to be 0.51 of a magnitude. This has been applied as a correction to the functions of the table, so that the

standard now assumed is, that *Polaris*, at a zenith distance of zero, shall have a photographic magnitude of 2.00.

Inasmuch as I used the same telescope and the same kind of plate, Seed 26, as were employed by Professor SHAEBERLE, the formulæ derived by him should be directly applicable to my work. In every respect I endeavored to follow Professor SCHAEBERLE'S methods in order to make the personal differences as small as possible.

The program of work consisted of making three exposures of two, four and eight seconds, which was later changed to four, eight and sixteen seconds, every ten or twenty minutes, for a period of several hours on each side of the computed time of minimum. All the exposures of one night were made upon the same plate, and kept as near as possible to the center. Several exposures of *Polaris*, of four, eight and sixteen seconds, were also made upon each plate, usually both at the beginning and the end of each night's work.

At first I used the measuring engine of the LICK Observatory for measuring the images; but this was unsatisfactory, for even with the lowest magnifying power the images were large and not sharply defined, so that it was impossible to make accurate measurements. I afterwards constructed a scale upon mica by drawing two very fine lines at a small angle to each other, and then making several similar triangles by drawing cross-lines perpendicular to the line bisecting the angle. The bases of the triangles were so placed that each differed from the preceding by $\frac{1}{1000}$ of an inch; so that, with a small magnifying power, it was possible to read off the diameter of an image to $\frac{1}{1000}$ of an inch. I always measured two diameters at right angles to each other. The results show that in general the images are round. All the measurements were made by lamplight, which was always, as nearly as possible, of the same intensity. All the measurements of any plate were made at one sitting, in order to avoid systematic differences which might otherwise enter.

The measured diameters of *Polaris* d_m generally differed slightly from the tabulated values d_t , and the correction, $\Delta d = d_t - d_m$, was determined for each plate and applied as a correction to each measured diameter of the variable, d_v . The resulting magnitudes of the variable were corrected for atmospheric absorption by means of the table upon page 86 of the work above mentioned. The time of minimum was then de-

terminated by plotting these corrected magnitudes and drawing a smooth curve through them.

Upon several nights, while photographing *Algol*, I also made visual observations. The comparison stars used were α *Persei* 1^m.9, γ *Andromeda* 2^m.1, β *Triangulæ* 3^m.2, and δ *Persei* 3^m.3. The magnitudes correspond to SCHÖNFELD's standard light scale, with the exception of α *Persei*, which was not used by him. ρ *Persei* was also used upon the first night, but as it is variable it was not employed upon subsequent occasions. The magnitude of ρ *Persei* was assumed to be 3.1. While making the visual observations, and also while measuring the plates, I took particular pains not to know the exact computed time of minimum.

The following table gives the minima determined. The time used is Pacific Standard time, 8 hours slow of Greenwich. The computed times of minimum are from CHANDLER's table, *Astronomical Journal*, Vol. VII, No. 167. The result of October 31 is given but half weight, as the zenith distance was very large at the time of minimum.

MINIMA OF *ALGOL*.

Photographic.				Visual.		$O_p - C.$	$O_v - C.$	$O_p - O_v$	
		D.	H.	M.	H.	M.	M.	M.	
1892.	Oct.	25	14	2.2	. . .	+ 47.8	. . .	. . .	
		28	10	39.6	. . .	+ 35.4	. . .	. . .	
		31	8	2.7	. . .	+ 69.6	. . .	. . .	
	Nov.	20	9	52.6	8	57.0	+ 77.5	+ 21.9	+ 55.6
	Dec.	7	14	25.5	14	3.0	+ 57.3	+ 35.8	+ 21.5
		10	11	0.5	10	33.9	+ 43.4	+ 16.8	+ 26.6
1893.	Jan.	2	8	48.1	9	4.6	+ 0.2	+ 16.7	- 16.5
MEAN						+ 45.6	+ 22.8	+ 21.8	

In most cases there is considerably more difference than one would expect between the observed photographic and visual minima. It is not likely that this difference is real, and as in every case except one, the visual minimum is nearer the computed, the inference is strong that the photographic method is affected by some uneliminated systematic errors. There is a large and consistent deviation of the visual minima from the computed ones, of which, however, I see no explanation.

In the following table I give in detail the observations of 1893,

January 2, in order to show what degree of accuracy has been obtained in the photographic method. I have chosen this date because at the beginning of the series I was inexperienced in photographic work, so that observations taken in the latter part may fairly be presumed to be more free from errors of inexperience and thus to better represent the true worth of the method. A mean solar chronometer set to Pacific Standard time was used in making the exposures. The chronometer correction was $-6^m 22^s$. The times given in column one are readings of the chronometer face. It took about a minute each time to make the exposures of four, eight and sixteen seconds. The beginning and the end of the operation were recorded and the time given is the mean of these two. The visual observations were made immediately after the exposures, so that a correction of about a minute should be applied to each of the recorded times of observation. This correction has been applied in deriving the visual minima. A second set of measurements of the images was made, with the same scale, more than a year after the first set. Both are given in the table for comparison. While making the second set of measurements, I had entirely forgotten the results obtained in the first set and took particular pains not to look at the first set until after the second was finished. For the first set $\Delta d = -0.0011$ inches and for the second -0.0006 . In some cases the differences are rather large and this shows that there is considerable uncertainty in such measurements. The resulting time of minimum, however, is changed only a few minutes.

TIME.	4 th EXPOSURE.		8 th EXPOSURE.		16 th EXPOSURE.		CORRECTED MEAN.		DIFFERENCE	VISUAL OBSERVATION.	VISUAL MAG.
	1st Set.	2d Set.	1st Set.	2d Set.	1st Set.	2d Set.	1st Set.	2d Set.			
6 50.5	1.74	1.84	1.57	1.79	1.73	1.67	2.14	2.23	M.	α 6δ, c 5δ	2 70
7 0.5	1.74	1.84	1.69	1.74	1.73	1.69	2.19	2.23	-0.04		
10.5	1.81	1.91	1.74	1.81	1.75	1.71	2.25	2.29	-0.04		
20.5	1.78	1.91	1.81	1.92	1.84	1.73	2.30	2.34	-0.04	α 7δ, c 6δ	2 82
40.5	1.88	1.94	1.92	1.95	1.90	1.84	2.40	2.41	-0.01	α 8δ, c 8δ	3 00
8 0.5	1.95	2.01	2.07	2.02	1.98	1.90	2.50	2.48	+0.02	α 9δ, c 9δ	3 12
10.5	2.18	2.15	2.15	2.05	2.05	1.98	2.64	2.57	+0.07		
20.5	2.35	2.18	2.20	2.05	2.11	2.06	2.73	2.61	+0.12	β δ, β δ	3 25
30.5	2.65	2.35	2.33	2.12	2.23	2.11	2.91	2.70	+0.21		
40.5	2.94	2.45	2.62	2.17	2.36	2.19	3.15	2.78	+0.37	δ 1β, β δ	3 30
9 0.5	2.77	2.56	2.40	2.40	2.25	2.26	2.97	2.91	+0.06	δ 1β, δ 1β	3.35
10.5	2.81	2.48	2.40	2.37	2.23	2.19	2.98	2.85	+0.13		
20.5	2.73	2.56	2.35	2.35	2.23	2.17	2.93	2.85	+0.08	δ 2β, β 1β	3 40
30.5	2.52	2.45	2.25	2.30	2.11	2.15	2.77	2.78	-0.01		
40.5	2.38	2.35	2.22	2.20	2.13	2.09	2.71	2.68	+0.03	δ 1β, β β	3.30
50.5	2.35	2.28	2.15	2.12	2.05	2.02	2.65	2.61	+0.04		
10 0.5	2.28	2.25	2.07	1.97	1.96	2.02	2.56	2.54	+0.12	δ β, β β	3 25
10.5	2.18	2.21	1.97	1.89	1.92	1.94	2.46	2.45	+0.01		
20.5	1.98	2.04	1.89	1.84	1.86	1.92	2.34	2.36	-0.02	α 9δ, c 8δ	3 07
30.5	1.91	1.91	1.81	1.81	1.77	1.86	2.24	2.27	-0.03		
50.5	1.74	1.81	1.74	1.76	1.73	1.80	2.11	2.16	-0.05	α 8δ, c 7δ	2 95
11 10.5	1.64	1.74	1.72	1.74	1.65	1.67	2.00	2.05	-0.05		
20.5	1.56	1.78	1.67	1.66	1.59	1.61	1.92	1.99	-0.07	α 8δ, c 7δ	2.95
40.5	1.58	1.74	1.61	1.66	1.58	1.61	1.86	1.94	-0.08	α 7δ, c 6δ	2 82
12 0.5	1.54	1.74	1.59	1.71	1.55	1.58	1.77	1.89	-0.12		
20.5	1.52	1.64	1.55	1.66	1.52	1.52	1.68	1.76	-0.08	α 6δ, c 5δ	2 70
50.5	1.44	1.61	1.51	1.59	1.47	1.47	1.51	1.60	-0.09	α 4δ, c 3δ	2 45
13 0.5	1.42	1.64	1.49	1.57	1.44	1.47	1.44	1.55	-0.11		

DISCUSSION OF RESULTS.

From the measured diameters of *Polaris*, there appears a systematic difference between Professor SCHAEBERLE's results and mine. This may arise from three causes: (a) Different development of plates; (b) Differences in the scales used; (c) Personal differences in the measurement of an image. The differences are probably largely due to the first cause. Following is a tabulation of the differences where $\Delta d = d_t - d_m$ as before stated:

	2 ^s Exposure.	4 ^s Exposure.	8 ^s Exposure.	16 ^s Exposure.
	IN.	IN.	IN.	IN.
Δd	— 0.0002	— 0.0006	— 0.0007	— 0.0007
r	$\pm$ 0.00017	$\pm$ 0.00028	$\pm$ 0.00026	$\pm$ 0.00029
i_o	$\pm$ 0.00003	$\pm$ 0.00003	$\pm$ 0.00003	$\pm$ 0.00004
n	24	63	64	45

There are also differences in the magnitudes determined from different exposure times. Each determination of the magnitude of the variable is the mean of three practically simultaneous exposures of different lengths. Theoretically, of course, the three exposures should all give the same magnitude. The following table will show the differences:

	Mag.	π .
2 ^s — 4 ^s	— 0.03	74
4 ^s — 8 ^s	+ 0.08	319
8 ^s — 16 ^s	+ 0.08	217

Here the probable errors are large and an inspection of the individual observations, especially the results of those plates which were measured twice, shows that the differences depend largely upon circumstances of measurement, so that the apparent systematic differences may be accidental. A photographic image is never perfectly definite in outline, so that the measurement of its diameter involves the judgment to a certain extent, and therefore the same images measured under different circumstances may yield quite different results.

Observations on a number of dates, when the star reached a considerable zenith distance, seem to show that the formula derived by Professor SCHAEBERLE for atmospheric absorption of photographic rays, gives corrections which are too large. In

1893, January 6, I made a series of observations to test the formula and found my suspicions strengthened. Upon that date *Algol*, which was not going through a minimum phase, was photographed every ten or twenty minutes from a zenith distance of 6° to $78^{\circ}.8$. The resulting magnitudes corrected by the formula for absorption should all be the same, but the following table shows considerable differences which increase with the zenith distance. In the first column is the zenith distance. In the second column is the resulting photographic magnitude of *Algol* corrected for absorption by the formula. The third column is the same as the second from an entirely independent measurement of the images. The fifth column gives the deviation from the formula on an assumption that the deviation at a zenith distance of 6° is insensible.

Z	Mag.	Mag.	Mean.	Error.
6	1.52	1.63	1.58	0.00
8	1.52	1.63	1.58	0.00
11	1.50	1.62	1.56	0.02
15	1.50	1.61	1.56	0.02
18	1.49	1.58	1.54	0.04
22	1.49	1.52	1.50	0.08
26	1.43	1.55	1.49	0.09
30	1.41	1.49	1.45	0.13
33	1.38	1.48	1.43	0.15
37	1.35	1.45	1.40	0.18
41	1.29	1.40	1.34	0.24
45	1.24	1.36	1.30	0.28
48.6	1.23	1.34	1.28	0.30
52.2	1.15	1.28	1.22	0.36
54.0	1.12	1.26	1.19	0.39
55.7	1.11	1.27	1.19	0.39
57.5	1.09	1.26	1.18	0.40
59.3	1.05	1.23	1.14	0.44
61.0	1.03	1.20	1.12	0.46
62.7	1.01	1.17	1.09	0.49
66.1	0.90	1.05	0.98	0.60
67.9	0.87	1.00	0.94	0.64
70.0	0.84	0.94	0.89	0.69
72.4	0.82	0.89	0.86	0.72
74.1	0.80	0.89	0.84	0.74
75.6	0.84	0.94	0.89	0.69
77.2	0.96	0.95	0.96	0.62
78.8	0.94	0.87	0.90	0.68

I intended to make some further tests of this formula, but my work was suddenly brought to a close at this point. The evidence, of course, is entirely too limited to condemn the formula, and I offer it as a suspicion. The differences are perhaps personal.

CONCLUSIONS.

If the above results can be taken as a fair sample of what may be expected in the photographic determination of variable star minima, then it is by no means certain that the new method is superior to the old one of visual observations. In the latter there are but two sources of error—that arising in estimating the differences between the variable and the comparison stars and that arising in the determination of the light scale of the comparison stars.

These particular sources of error do not occur in photographic observations, but others of perhaps as serious or more serious nature do enter. They may be enumerated briefly as follows :

First.—The magnitude is a function of the time of exposure. As the exposures are usually made by removing and replacing a cap at the objective end of the telescope there is always some uncertainty as to the time, even when the observer has a time-piece to beat seconds. This, however, need not be looked upon as a serious source of error.

Second.—It is very necessary to have a perfectly clear sky, as the presence of the least haze or clouds will vitiate the results more than in visual observations.

Third.—A perfectly uniform development of the plates, not an easy thing to get, is necessary to complete uniformity of results.

Fourth.—Perhaps one of the most serious difficulties is the lack of uniformity in the sensitiveness of the photographic film. I have found that with the best plates, and even at the distance of only one or two inches from the center, there are sometimes annoying changes of sensitiveness, which may introduce large discrepancies in the resulting magnitudes. Plates which may be very good for ordinary purposes may be entirely worthless for such work as this, for when a change of one ten-thousandth of an inch in the diameter of an image may make a change of one-tenth of a magnitude, it is easy to see that very slight changes of sensitiveness may cause large discrepancies in the results.

Fifth.—The images are always indefinite in outline, and the measured diameters are therefore dependent upon the judgment of the observer. By care and continued practice, however, the

mode of measurement may be brought to a system, and thus all but systematic errors eliminated.

Sixth.—The formulæ for determining the magnitude and for giving the atmospheric absorption are entirely empirical. Those determined for any one telescope may be worthless for another telescope, or for the same telescope with a different plate, or even perhaps for the same telescope and plate, but a different observer. Personal differences enter so largely into the formulæ that I am inclined to the belief that they can be used with entire confidence only by the person by whom they were determined.

Some of these sources of error can be controlled by the use of extreme care, and the photographing of *Polaris* upon each plate helps to eliminate some of them.

In setting forth the above sources of error it has not been my purpose to condemn or to disparage the photographic method, but merely to point out some of the difficulties encountered. I am thoroughly convinced, however, that in order to obtain perfectly reliable results it is necessary to make the work entirely homogeneous, not only to the extent of always using the same telescope and plates, but to the extent that each observer must determine for *himself* formulæ for photographic magnitudes and atmospheric absorption. If this be done, and if a more perfectly sensitive film can be manufactured, then I believe that more reliable observations of the changes of short-period variable stars could be obtained than by the visual methods.

Dr. CHARLIER, of Stockholm, has published a small pamphlet, giving his photographic observations of a minimum of *Algol* in 1891, November 2. His results are more accordant than mine, and his photographic and visual minima are in a very close agreement. He finds a photographic minimum of 2.74 magnitude and a visual one of 3.4 magnitude, with a range of but 0.7 of a magnitude between the maximum and minimum photographic brightness. My observations give an average photographic minimum of 2.45 magnitude, and an average visual minimum of 3.33 magnitude. The photographic range, however, exceeds one magnitude.

Dr. CHARLIER expresses entire confidence in the photographic method, and thinks that under proper atmospheric conditions much better results can be obtained by the photographic than by the visual method.

DETROIT OBSERVATORY,
ANN ARBOR, Michigan.

REMARKS ON THE "PHOTOGRAPHIC OBSERVATIONS OF *ALGOL*."

By J. M. SCHAEBERLE.

With reference to the cause of the apparent discordance between Mr. TOWNLEY's observed results and my published tabular values, I wish to call attention to the fact that since the publication of my memoir* another determination, based upon a large number of observations, has been published by the Harvard College Observatory.† As will be seen from the comparison given below, the Harvard College Observatory values, at 80° zenith distance, are practically the same as my own LICK Observatory values :

ZENITH DISTANCE.	ABSORPTION.		DIFFERENCES.	
	L. O.	H. C. O.	L. O.—H. C. O.	L. O.—T.
°	M.	M.	M.	M.
73.4	1.34	1.10	+ 0.24	+ 0.73
75.4	1.48	1.26	+ .22	+ .69
77.4	1.66	1.44	+ .22	+ .62
79.4	1.86	1.88	— .02	+ .68

Mr. TOWNLEY's results (L.O.—T.) for the same zenith distance would, therefore, seem to be in error something more than half a magnitude.

Aside from purely personal errors it is unfortunate, to say the least, that, in the single special set of observations made, the observer selected a variable star to test my values for absorption.

LICK OBSERVATORY, July 27, 1894.

* "Terrestrial Atmospheric Absorption of the Photographic Rays of Light."

† "A Photographic Determination of the Atmospheric Absorption" *Annals Harvard College Observatory*, Vol. XIX).

STELLAR PARALLAX.

BY PROFESSOR F. P. LEAVENWORTH.

Since the time of KEPLER, numerous attempts have been made to detect the parallaxic displacement of the stars. The earlier attempts were unsuccessful because the instruments employed were far too crude to detect so minute a displacement. Of the many methods tried, there are undoubtedly four, and perhaps six, which may be relied upon to yield real values for the parallax where the latter is not smaller than one-tenth of a second. These are :

- (1) Observations of zenith distance with a zenith instrument, for absolute parallax.
- (2) Observations of Right Ascension with a transit instrument.
- (3) Observations of difference of Right Ascension with a transit instrument.
- (4) Observations with a heliometer.
- (5) Observations with an equatorial.
- (6) The photographic method.

Until recently the results obtained with the meridian instruments have not been encouraging. The large probable errors and the discrepancies between different observers show the results, in most cases, to be practically worthless. However, the recent methods used by Dr. BELOPOLSKY and especially by Professor KAPTEYN, give results which are apparently comparable with the best values of the heliometer.

It is now my purpose to discuss these methods with the hope of arriving at some conclusion as to which is best adapted to the problem of parallax. In comparing them, these three criteria will be employed as far as possible:

First.—Accuracy of an observation.

Second.—Freedom from unknown constant errors.

Third.—Rapidity and convenience of observation.

Probably the best means of comparing the accuracy of the different methods is by means of the probable errors of single observations, provided the time spent in making the observation

is considered. The following probable errors have been taken from the best authorities at hand, and are probably not far from the best that can be done with the respective instruments:

(1) Zenith distance,	$\pm 0''.4$
(2) Observed Right Ascension,	± 0.3
(3) Difference of Right Ascension,	± 0.25
(4) Heliumeter,	± 0.20
(5) Equatorial,	± 0.10
(6) Photography,	± 0.15

From this it seems that observations with the equatorial give the smallest probable error. However, this small value is only for moderate distances of the comparison star.

Another test of the accuracy may be made by comparing the average probable error of the final value of the parallaxes. The following table gives the comparison between the results for the six methods, using all the good observations which have been made.

	No. of Stars Used.	Average Probable Error.
(1) Zenith distance,	7	$\pm 0''.09$
(2) Observation of R. A.,	9	0 .08
(3) Difference of R. A.,	16	0 .03
(4) Heliumeter,	33	0 .02
(5) Equatorial,	25	0 .04
(6) Photography,	27	0 .03

In this test the heliometer appears to have the advantage; but there is not much choice between the last four methods. However, the test is not perfectly fair, as the number of measures going into each determination of parallax is not considered, being not at hand. In fact, it is probable that the above superiority of the heliometer is partly due to the greater number of observations used in determining the parallax of the first magnitude stars. Evidently the last four methods may be relied upon to furnish a real value of the parallax, provided there is no constant error to vitiate the results, and provided the real parallax is not less than one-tenth of a second.

That constant errors exist is lamentably evident. One has only to glance over a list of parallax results to see that there are great discrepancies. As examples of these, OTTO STRUVE obtained for α *Tauri* a parallax of $0''.52 \pm 0''.06$, while ASAPH HALL obtained for the same star $0''.10 \pm 0''.03$. Again, BALL's

value for 6 (B) *Cygni* is $+0''.48 \pm 0''.05$, where HALL obtains $-0''.02 \pm 0''.01$. When, however, we consider the minuteness of the angles measured, the results taken as a whole are really in fine accord. Out of ninety measures made on twenty-three stars, sixty-four differ from the arithmetical mean (for each star) less than one-tenth of a second.

There seems to be, in equatorial measures, a greater tendency to constant error, than in the other best methods. Where the greatest differences between results are found, usually the equatorial has been employed. When also two co-ordinates are measured by the same observer, the results very frequently do not agree. To be sure the other methods can not be exposed to the same test, for usually only one co-ordinate is measured. Where several comparison stars are observed with the heliometer, or by photography, the agreement is much better. There is, however, sufficient discrepancy between the results to cause us to question whether the differences are due to constant error or to parallax in the comparison star itself. Repeated measures by different observers will make evident to what extent the parallax is diminished by the parallax of the comparison star.

For rapidity of observation the meridian method, at first glance, appears to offer the most decided advantages, as it takes only a few moments to make the observation as the star crosses the meridian. It must be remembered, however, that the observer must wait until the star is on the meridian, and unless a large number of stars are included in his list it will be necessary for him to spend considerable time in waiting. Practically, however, the same inconvenience should be found in all observations of parallax, since it is always advisable to make the observations at about the same hour-angle. The photographic method offers decided advantages in time saving. The time of exposure is usually about five minutes. If two exposures are taken, as is customary, there will be a little over ten minutes given to the actual observation, and the plate will contain the images of a number of comparison stars.

Again, photography and the heliometer give the greatest latitude for choosing desirable comparison stars. There is, however, this difficulty in the photographic method, that the comparison stars must be selected in groups of two, symmetrically placed with regard to the primary star.

To sum up, therefore, the first or absolute method of observ-

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ing zenith distances has up to the present time furnished few reliable observations. More may be expected of it since the discovery of the periodic variation in latitude.

The second method of observing absolute Right Ascensions appears to be lacking in accuracy, except when applied to stars very near the pole. It has this advantage, however, that even though the measures prove worthless for parallax, they should give fine values of Right Ascension.

The third method consists in observing with the transit instrument the difference of Right Ascension between the star and suitable comparison stars. A chronograph is used for observing the time of transit and various precautions taken. By this means Professor KAPTEYN has obtained results that agree well with other measures, and have small probable errors.

Professor HALL has measured the difference of Right Ascension with the equatorial, by means of the micrometer, with the driving clock running, and has obtained good results. Where the comparison star is so distant that the micrometer cannot be used, the difference of time, as in comet measures, might be employed. The difference of Declination may also be measured with the micrometer of the transit instrument, just as is done with the equatorial.

The fourth method—by means of the heliometer—has been considered, until recently, by far the best method for determining parallax. It holds this high position for the reason that comparatively large distances can be measured with as great an accuracy as small ones. The probable error of a distance of 3000'' seems no greater than for a distance of 30''. For this reason the number of available comparison stars is greatly extended, and the convenience of selection greatly increased. However, the rarity and cost of this instrument is against it. Also, on account of the smallness of the object-glass, the fainter comparison stars cannot be used.

The equatorial has many points of advantage and some of disadvantage. The great objection, perhaps, is the difficulty of obtaining a number of suitable comparison stars. In many cases a near companion cannot be found. Very seldom are there two or more companions. Now, the accuracy of observation is an inverse function of the distance, so that observers (making, perhaps, an exception of Professor HALL) cannot usually obtain sufficiently accurate measures where the distance is much greater

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than 100". If suitable comparison stars were clustered within 30" of the star, the equatorial method would furnish unequalled advantages. Where proper motion has been discovered in the brighter component of a double star, observations of position angle and distance give unequalled accuracy. There should be no objection to the comparison star under such circumstances. The angular nearness of the star is accidental. The proper motion, in course of time, will make the companion a distant object.

The photographic method is considered by many to be the coming method, if indeed it is not already the *present* method. The results obtained by RUTHERFURD-JACOBY and by PRITCHARD seem equal to the best; where their results are compared with other measures there is usually a close agreement, and, where no other measures have been made, the photographic parallax is almost without exception very small—a strong indication of accuracy. However, there are two circumstances which mar the otherwise perfect beauty of their results. They are, first, the discrepancy between PRITCHARD'S and JACOBY'S values for the parallax of μ *Cassiopeia*; second, the large parallax obtained by JACOBY from two RUTHERFURD photographs of β *Cygni*. The latter parallax was found to be erroneous, by recent heliometer measures made at Yale University. For the parallax of μ *Cassiopeia* it curiously happens that two other observers agree, one with PRITCHARD, the other with JACOBY, as follows :

OTTO STRUVE,	$0''.33 \pm 0''.06$
RUTHERFURD-JACOBY,	0.28 ± 0.02
SCHWEIZER,	0.08 ± 0.06
PRITCHARD,	0.03 ± 0.02

The apparent accuracy of these photographic results is surprising. One would think that in bisecting large and ill-defined spots, as the brighter stars appear to be under the microscope, large probable errors and constant errors also would be likely to occur. However, Professor PRITCHARD'S measures show that bisections can be made with a probable error less than $\frac{1}{10}$ of the diameter of the image. Thus, a spot 20" in diameter may be bisected with a probable error of two-tenths of a second.

To my mind the real advantage of this over other methods lies in the convenience and accuracy with which comparatively

large distances may be measured. It is like the heliometer in this respect, with the added advantage that in fine weather a large amount of material may be stored away to be worked up at leisure.

UNIVERSITY OF MINNESOTA, July, 1894.

CAN ORGANIC LIFE EXIST IN THE SOLAR SYSTEM ANYWHERE BUT ON THE PLANET *MARS*?

[A letter from a citizen of *Mars*, found in a meteorite which fell in the rooms of the A. S. P.]

Communicated by M. CAMILLE FLAMMARION.

In the following discussion I hold to the principle that, in explaining any phenomena taking place elsewhere than in *Mars*, we must not assume the existence of new and unknown forces and properties of matter. The more our knowledge of the universe progresses the more we become convinced that the phenomena observed on *Mars* are only repeated, on a smaller or a grander scale, in every nook and corner of infinite space.

One of the very first points to be examined in considering the conditions of habitability of a world, is evidently the question of the stability of beings and of things, and at the same time the allied question of their freedom to move about. It is essential that a man must not be too heavy, nor yet too light. Too great levity would prevent all fixity in the work of his hands—and perhaps all stability in his character, even. Too great gravity would chain him to the soil. It is essential that all useful or agreeable things, dwellings, furniture-- everything, in short—should be in harmony with the muscular force of a man, with his stature, his weight, etc. Here, for example, an object falling from the top of an edifice traverses 1.84 mètres during the first second of its fall.* This is a necessity of the first importance. Suppose, for instance, that bodies, instead of falling with this safe slowness, were attracted by a violent force and were precipitated in a brutal descent two or three times more rapid! In such a case it would be impossible to build houses without

* The editor has translated the Martian measures into terrestrial ones.

danger to life, and men could no longer risk the navigation of the atmosphere! Our astronomers have taught us that on the Earth, for example, gravity is so intense that bodies fall with a velocity of 4.90 mètres during the first second of their descent. This perpetual menace of death to whomsoever would attempt to quit the soil, from a desire to elevate himself towards some superiority, alone suffices to prove to us that the Earth is uninhabitable by an intelligent race. At the best, some vile and crawling creatures may exist there. A Martian man who weighs 100 kilogrammes, would there weigh 266. Life would be impracticable. He could not move.

A second condition for the habitability of a world, not less important, is, assuredly, its distance from the Sun, that source of all light, heat and life. When one reflects on the narrow limits within which life manifests itself, when one knows that all beings are benumbed by a little too much cold, and suffocated by a little too much heat, one cannot be too careful to keep Nature enclosed within her bounds. *Mars* is exactly at the providential distance. Here it is never too warm nor too cold. The snow-fields of our polar regions melt in summer, as is proper, and are formed once more in winter to feed our water-courses. If *Mars* could be removed to the distance of *Jupiter* we should all be instantly frozen to death. If, on the other hand, our planet were to approach the Sun to the regions where the Earth revolves, it would then receive more than twice the heat that is now received, and our fields would be parched. Every one knows how intolerable and dangerous are, even now, certain summer heats. Think what it would be were you exposed to a Sun more than twice as hot as our own! Wretched Earth! If, by mischance, it is inhabited, it can only be by salamanders* or by passionate creatures of some sort. The most elementary common sense shows that the other planets are either too near or too far from the Sun and that our own is alone at the golden mean.

Since we are discussing the question of the habitability of other worlds by intelligent beings—the only kind of beings of any interest to us—let us also consider the density of the substances which enter into the constitution of bodies. No one can think of denying that the Martian human form is the only perfect one, and that intelligence could not choose its domicile in any heads

* The editor has translated the Martian words into their terrestrial equivalents.

except ours. We are supremely complete, even to the point that artists, wishing to represent God in our sanctuaries, have figured Him in the image of a Martian man. Well—this elegant form, this elevated stature, these aerial wings, these eyes which respond to the ultra-violet radiations, all of these could not exist on other globes, on account of their different conditions. Consider, for example, the Earth. Its density is a full third too great. Everything on it is much too heavy. Terrestrial organisms could not resemble ours, and would be species of monsters. Moreover, the eye, that essential organ, if developed in a too intense light, would be incapacitated to respond to the ultra-violet rays, and would be blind to more than half of the multitude of things. Under whatever aspect we look at it, the Earth is, then, an uninhabitable world. The same is true of the other planets, for they differ too greatly from ourselves.

Moreover how could one live usefully in a world where the years pass so quickly? During the period in which one of us attains the middle age of fifty years, those on the Earth have become decrepit old men of ninety-four; if indeed they are not already dead. What time would there be for good or useful things in a rapid whirlpool like that? Their days are also shorter than ours, though only by 39 minutes—hardly worth mentioning. But the matter of the years is much more serious, for the average duration of a human life is a considerable factor in the history of progress. If then the Earth might possibly be inhabited, this would be relatively useless, for it would revolve indefinitely in the same circle of prejudices. There would be no time to acquire the experience which would be so useful to succeeding generations.

How gross must be the alimentation of living beings on the Earth. Celestial chemistry teaches us that the terrestrial atmosphere is not nutritive. Are we obliged to degrade the men on the Earth by supposing them to have alimentary canals, like our inferior animals? and to suppose that they must continually slaughter beasts that they may feed on their corpses? No—we will stop short of that. But, assuredly, the mode of alimentation, whatever it be, cannot be compatible with the requirements of an advanced intellectual state; so that if the Earth be inhabited at all, it can only be by animals of a distinctly low order.

Moreover with what senses may we dare to endow organisms immersed in such surroundings? Touch, smell, hearing, perhaps vision of the most brilliant of the spectral rays. What

would they make of our seventeen senses? The sense of magnetic direction will certainly be wanting; and their dull intelligences will surely be incapable of communicating by psychic force alone. Unquestionably they can enjoy no more than five or six senses. A trifle! What a wretched condition!

Thus, from whatever side we consider the question, we find conditions opposed to our own. Even the simplest cosmographic arrangements are lacking. For instance, we have two Moons whose motions are so marvellously combined that the first rises in the west, while the second rises in the east; and both of them move so rapidly across the sky that they serve us as a perpetual time-piece in every circumstance of life, and even enable our sailors to calculate instantly the positions of their ships. Well—the Earth has but a single satellite which turns with ridiculous slowness and which can serve no useful purpose.

And again, why speak of the stars to the supposititious inhabitants of the Earth? Here we have nearly continual fine weather, while there the sky is constantly covered with clouds scattered here and there. Where shall the inhabitants live? Upon these clouds? That would be a fragile foundation. Beneath them? But there they could see nothing of the sky. And, furthermore, the terrestrial atmosphere is of an excessive density. It is a veritable sea, in whose depths we can only imagine fishes to exist. All these reasons, and a million others which might readily be added, agree in establishing the conclusion that the Earth is uninhabitable, and that the same is true for the other planets, for all of them differ from our admirable dwelling-place. The extravagant idea of a plurality of worlds is a pure chimera, unworthy the attention of a Martian.

In concluding this investigation we cannot help admiring the inductive acumen of the theologians who considered our *Mars* to be the most important of the planets and the center of creation. Although their opinions were not based upon scientific facts, they arrived at the truth, nevertheless.*

ATTEST: a true copy.

CAMILLE FLAMMARION.

* Translated by EDWARD S. HOLDEN.

(SIXTEENTH) AWARD OF THE DONOHUE COMET-MEDAL.

The Comet-Medal of the Astronomical Society of the Pacific has been awarded to WALTER F. GALE, Esq., F. R. A. S., of Paddington, N. S. W., for his discovery of an unexpected comet on April 2, 1894.

The Committee on the Comet-Medal,

EDWARD S. HOLDEN,
J. M. SCHAEBERLE,
CHAS. BURCKHALTER.

June 2, 1894.

CLOUD OBSERVATIONS ON THE ISLAND OF YEZO, JAPAN, WITH REFERENCE TO THE SOLAR ECLIPSE OF 1896, AUGUST 9.

BY W. W. CAMPBELL.

Through the kindness of Professor HIROI of Sapporo, Japan, Professor HOLDEN has received copies of the meteorological observations made at five stations on the island of Yezo, which lie in the path of the eclipse which occurs 1896, August 9. The observations extend back only to 1889 or 1890; but for the three or four years covered they are very complete. They were made at 4-hour intervals throughout the day. For the benefit of those who are planning to observe the eclipse, I have extracted from the records the average cloudiness, for the months of July and August, at 2 P. M. and 6 P. M. (The eclipse occurs at about 3 P. M.) The results, printed below, are expressed on a scale of 10 : 0 being perfectly clear, and 10 completely clouded :

SOYA METEOROLOGICAL STATION.

Long. 141° 50' E. Lat. 45° 30' N.

	1890.		1891.		1892.		1893.		Mean.
	2 P.M.	6 P.M.	2 P.M.	6 P.M.	2 P.M.	6 P.M.	2 P.M.	6 P.M.	
July...	6.5	7.2	7.1	7.7	6.4	6.1	7.9	8.2	7.1
Aug...	5.5	5.8	7.6	7.5	7.2	7.4	7.8	7.6	7.0

KAMIKAWA METEOROLOGICAL STATION.

Long. $142^{\circ} 20'$ E. Lat. $43^{\circ} 45'$ N.

	1889.		1890.		1891.		1893.		
	2 P.M.	6 P.M.	2 P.M.	6 P.M.	2 P.M.	6 P.M.	2 P.M.	6 P.M.	Mean.
July...	7.3	8.1	6.7	6.2	8.3	7.9	7.8	7.4	7.5
Aug...	5.9	5.7	5.3	4.6	7.8	7.4	7.5	7.8	6.5

ABASHIRI METEOROLOGICAL STATION.

Long. $144^{\circ} 15'$ E. Lat. $44^{\circ} 0'$ N.

	1890.		1892.		1893.		
	2 P.M.	6 P.M.	2 P.M.	6 P.M.	2 P.M.	6 P.M.	Mean.
July.....	6.4	7.6	8.4	8.4	7.2	7.7	7.6
Aug.....	4.2	3.5	7.1	7.0	7.3	8.3	6.2

KUSHIRO METEOROLOGICAL STATION.

Long. $144^{\circ} 35'$ E. Lat. $43^{\circ} 20'$ N.

	1890.		1891.		1892.		1893.		
	2 P.M.	6 P.M.	2 P.M.	6 P.M.	2 P.M.	6 P.M.	2 P.M.	6 P.M.	Mean.
July...	6.0	7.4	8.3	8.5	7.5	8.4	..	..	7.7
Aug...	8.0	9.6	7.8	8.5	7.3	8.7	7.3	8.6	8.2

NEMURO METEOROLOGICAL STATION.

Long. $145^{\circ} 35'$ E. Lat. $43^{\circ} 20'$ N.

	1890.		1891.		1893.		
	2 P.M.	6 P.M.	2 P.M.	6 P.M.	2 P.M.	6 P.M.	Mean.
July.....	6.6	8.2	7.5	8.5	7.8	8.6	7.8
Aug.....	7.6	8.7	7.9	8.1	6.9	8.1	7.9

While these observations, extending over only three and four years, are too few to fix the average cloudiness, they are sufficient to give a general indication of the chances for clear weather. The eclipse occurs on August 9; and probably the best estimate of the cloudiness for that date will be obtained from the above data by combining the means for July and August, giving double weight to those for August. We obtain by that method, for

Soya,	7.0
Kamikawa,	6.8
Abashiri,	6.7
Kushiro,	8.0
Nemuro,	7.9

These results make it plain that the chances for clear weather are considerably greater in the northwest part of the island than in the eastern part. This is explained by the fact that the province of Nemuro and the eastern part of the province of Kushiro are, according to Professor HIROI, " *Very* foggy during the summer months !"

Astronomers will be gratified to learn that the interest of the Japanese Government in the eclipse has been enlisted. Professor HIROI writes : " Our Department of the Interior issued instructions to county offices lying within 30 miles on each side of the line joining Soya and Akkeshi Bay to make cloud observations during the summer months until 1896." The results can probably be obtained, as fast as they are collected, from the Central Meteorological Observatory at Tokio.

PREDICTIONS FOR THE TRANSIT OF MERCURY, NOVEMBER 10, 1894.

BY ORRIN E. HARMON.

TIMES OF INGRESS AND EGRESS FOR SEATTLE, SPOKANE AND WALLA WALLA, WASH., AND PORTLAND, OR.

SEATTLE.		SPOKANE.		WALLA WALLA.		PORTLAND.	
Lat. 47° 35'		Lat. 47° 40'		Lat. 46° 6'		Lat. 45° 32'	
Long. 8 ^h 9 ^m 20 ^s		Long. 7 ^h 49 ^m 40 ^s		Long. 7 ^h 53 ^m 36 ^s		Long. 8 ^h 10 ^m 52 ^s	
INGRESS.	EGRESS.	INGRESS.	EGRESS.	INGRESS.	EGRESS.	INGRESS.	EGRESS.
A. M.	P. M.	A. M.	P. M.	A. M.	P. M.	A. M.	P. M.
P. S. T.	P. S. T.	P. S. T.	P. S. T.	P. S. T.	P. S. T.	P. S. T.	P. S. T.
H. M. S.	H. M. S.	H. M. S.	H. M. S.	H. M. S.	H. M. S.	H. M. S.	H. M. S.
7 56 01.58	1 12 27.70	7 56 00.32	1 12 25.78	7 56 01.2	1 12 25.54	7 56 02.52	1 12 27.10

CHEHALIS, Lewis Co., Wash., June 21, 1894

THE AREQUIPA OBSERVATIONS OF *MARS* AND
JUPITER.

BY PROFESSOR W. H. PICKERING.

In the *Publications* A. S. P. for June, under the title "The Lowell Observatory in Arizona," Professor HOLDEN criticises the observations made by myself and my assistant in Arequipa in 1892. My reply is based upon an examination of a proof-sheet which he has very kindly forwarded to me. As far as the criticism relates to the planet *Mars*, it appears to be at least in part a personal one. Personal controversies seem to me to be ill-suited to the pages of an astronomical magazine, but under the circumstances, I may perhaps be allowed a few words in reply, before dealing with other matters.

It is true that two years have already elapsed, and a full report of our observations has not as yet appeared. This is, I presume, also the case with the work of the LICK Observatory, since I have thus far seen only fragmentary articles by the various members of its corps of astronomers. I am in hopes that a full report upon our work, with illustrations, will appear in less than twelve months.

Fragmentary articles have also appeared, by the Arequipa observers, and it seems to me a little strange that Professor HOLDEN should not have referred to them, instead of criticising the necessarily imperfect accounts of our work which were transmitted by cable. As is perfectly well known, a cable despatch consists merely of a disconnected series of words, the grammatical construction and finish being supplied by the newspaper staff. Some mistakes were almost necessarily made in deciphering the despatches, such as the one pertaining to "trouble in the Arean clouds," cited by Professor HOLDEN, but as a whole I think that the *Herald* staff did their work remarkably well. Nevertheless, I do not expect to hold myself responsible for all the details of the wording of the telegrams, when precisely the same facts are stated in my own words in so well-known a periodical as *Astronomy and Astro-Physics* (Vol. XI, 1892, pages 668 and 849). The cablegrams were sent to inform other astronomers and the general public of the character of the work that we were doing, and to call the attention of the former to certain facts that we had

observed, in the hope that they might confirm them while the planet was still near to us. A more detailed report was given later, as above noted, and a full report of all our observations will appear before long in a volume of the *Annals of Harvard College Observatory*.

As an instance of the injustice of basing a criticism of the Harvard work upon a mere cable despatch, let us take Professor HOLDEN's first statement. He says: "'The *Herald* correspondent at Valparaiso cables as follows: 'Professor PICKERING of the Harvard Branch Observatory at Arequipa says that he discovered forty small lakes in *Mars*.' How does he know the dark markings are lakes? Why does he not simply call them dark spots? And is he sure there are forty?'" Now, if Professor HOLDEN had examined my article upon the subject in *Astronomy and Astro-Physics* for 1892, page 850, he would have read: "Scattered over the surface of the planet, chiefly on the side opposite to the two seas, we have found a large number of minute black points. * * * * Over forty of them have been discovered, and for convenience we have termed them lakes." We should have liked to have put all this into our cablegram, but instead we simply said "forty lakes." It does not seem to me that the word lake is objectionable. It has already been applied by SCHIAPARELLI to several of the larger dark areas upon the planet, *Solis Lacus*, *Lacus Phœnicis*, etc. The forty smaller bodies discovered in Arequipa differed apparently from those mentioned by SCHIAPARELLI, chiefly in size.

The same remarks apply to Professor HOLDEN's criticism of the mountain chains. In the above-mentioned article, upon page 851 he will find the statement: "At present the snow is divided into two sections, one long and narrow, the other of irregular shape, and somewhat mottled. The appearance is such as might be produced by a mountain range and an area of irregular elevation, with a valley lying between them."

Regarding Professor HOLDEN's criticism of our cloud observations, I may state that upon August 5 and 6, and also upon September 10, 12 and 13, small portions of the *limb* were seen to project beyond the general contour of the planet. As I understand it, the only previous similar observation was made at the LICK Observatory, and was due to the discovery of a visitor, who noticed a white point projecting beyond the *terminator*. (*Publications A. S. P.*, Vol. VI, page 104.) Upon September 13, I

recorded: "7^h 05^m Arequipa Mean time. Two distinct elevations not less than 0".1, nor more than 0".2. * * * Tried 700 diameters. Think projections do not exceed 0".1, but cannot be much less." Later Mr. DOUGLASS observed them, and at 10^h 15^m I write: "The northern projection is very conspicuous, and before Mr. DOUGLASS had told me what its peculiarity was, I thought I could possibly distinguish a dark line separating it from the planet. * * *" This dark line is well shown in a sketch made by Mr. DOUGLASS at 9^h 33^m.

Neither do I consider Professor HOLDEN's effort to disparage the Harvard drawings, as compared to those made at the LICK Observatory, justifiable. I did not send finished india ink sketches to *Astronomy and Astro-Physics*, but merely line drawings, knowing that they could be published more cheaply, and yet would serve to illustrate my remarks. The Harvard drawings were not selected with a view of showing what fine detail we could see, but merely to show the changes exhibited by a certain region upon closely successive nights. The *Solis Lacus* region of *Mars*, sketches of which are exhibited by Professor HOLDEN, is the region showing perhaps the greatest amount of fine detail to be found anywhere upon the planet, and I think that our drawings of it indicate quite as many lakes and canals as do the drawings published by the LICK Observatory. In any case comparisons are odious, and I prefer to let our drawings, when published, speak for themselves, rather than to discuss the matter further at present.

Regarding the Arequipa observations of *Jupiter's* first satellite, the facts of especial note that we derived were two-fold:

(a) We stated that the satellite had the form of a prolate ellipsoid, or, popularly speaking, was egg-shaped.

(b) We stated that, unlike our Moon, it rotated upon its axis in a period different from that of its revolution around its primary.

Turning now to the *Publications A. S. P.*, Vol. III, pages 355 and 359, we find that Professors SCHAEBERLE and CAMPBELL announce that Satellite I "is ellipsoidal, that its largest axis is directed toward the center of *Jupiter*." That is, they appear to agree with statement (a), but deny statement (b). These results are quoted from Professor HOLDEN's paper in *Astronomy and Astro-Physics*, 1894, page 357.

On the other hand, in *Monthly Notices*, Vol. LIV, page 135,

and again in *Astronomy and Astro-Physics*, 1894, page 274, Professor BARNARD, also observing with the 36-inch telescope, denies statement (a), but apparently confirms statement (b), for he says: "From peculiarities in the appearance of the belt, it is probable that the period of rotation on this axis is not coincident with the satellite's period of revolution about *Jupiter*."

Now, with such discordant statements emanating from three such eminent observers, all using the same telescope. I think many persons would be interested if Professor HOLDEN would state definitely what he considers to be the verdict of the great LICK telescope upon these two disputed points. As far as Mr. DOUGLASS and myself were concerned, using our Arequipa instrument, our results were always concordant upon these points, and appeared to each of us at the time unmistakable. Of course, we neither claim superior eyesight, nor an equal instrument. Can the difference be due to the atmosphere, which at Arequipa was almost always very steady? I can offer no other explanation of the circumstance, and merely state what appeared to us to be the facts.

But one point remains to which I wish to draw attention. Regarding the shadows of *Jupiter's* satellites, Professor HOLDEN says: "They are seen at certain times to be twice as long as they are broad, and this phenomenon is regularly observed here. I believe it was not noticed in Peru, which is strange, considering how much more difficult observations are reported." Why Professor HOLDEN should say that this phenomenon was not noticed in Peru, or where he got his information, I cannot conceive. I certainly do not remember ever writing anything which could convey such an impression. In point of fact, the appearance was so easy to perceive, and its explanation was so obvious, that we rarely even mentioned it in our records. In looking over my observations, however, I find a few notes upon the subject, among others, under date of November 18, 1892, the entry, "8^h 06^m.—A very elongated shadow has just appeared upon *Jupiter*." This proved to be the shadow of the second and smallest of the four original satellites. A later entry, under the same date, reads "8^h 30^m.—Shadow of the second is equatorially elongated, but not so much so as it appeared when first seen on the limb of the disc." This phenomenon was apparently first observed and explained by an amateur, Mr. BARNEBY, of England, who, on April 5, 1861, recorded the change of shape

of the shadow of the third satellite, from an elongated streak to a circular spot. (*Chambers' Astronomy*, 4th ed., Vol. I, page 190.)

In making this reply to Professor HOLDEN's criticism of my work, I have taken up in sequence all the points raised that seemed most important to me, and treated them as briefly as possible. I hope that no further discussion of the matter will be necessary. Professor HOLDEN, as he truly remarked, spoke courteously, but very plainly. I am glad that he did so, as it has given me an opportunity to endeavor to do the same. As I am sure he meant no personal offense in making his somewhat severe criticisms of the Harvard observations, so I in turn trust he will take none at my rejoinder.

ADDENDUM.—THE AREQUIPA OBSERVATIONS OF
MARS AND JUPITER IN 1892.

BY EDWARD S. HOLDEN.

I agree with Professor PICKERING that no further discussion of the Arequipa observations is now desirable, and the following paragraphs are only designed to make certain points quite clear before leaving them. I acknowledge the courteous tone of Professor PICKERING's reply, and desire, as he does, to treat the matters in question entirely from a scientific standpoint.

Professor PICKERING entirely misses the important point of my paper. I criticised the large assertions of his cable-telegrams of 1892* (which were addressed to the newspaper readers of the whole world), because he announced various things, which were doubtful then and which he admits are not certain now, with a positiveness which was misleading and unwarranted.

He even went so far as to prophesy that certain melted snow would, by and by, flow in a particular direction. How could he possibly know which way was down-hill?

* I quote part of a single one of these, as follows, and call attention to its very definite statements:

"*Mars* has two mountain ranges near the south pole. Melted snow has collected between them before flowing northward. In the equatorial mountain range snow fell on the two summits on August 5 and melted on August 7. These clouds (on *Mars*) are not white."

Professor PICKERING says these telegrams were partly intended for the general public. If so, I maintain that it was indispensable to explicitly state that they were simply his provisional interpretations of "appearances," not the accepted dicta of science. When Professor PICKERING wrote his articles for scientific publications the positiveness of his cable-telegrams vanished. He did not there insist upon any explanations of his observations, but only on the accuracy (*i. e.*, the faithfulness) of the observations themselves;† he says, guardedly, that certain dark regions are "presumably water."‡ The expression of the telegrams: "*Mars* has two mountain ranges," becomes "the appearance is such as might be produced," etc.; the "region between them," becomes the "supposed valleys," etc.||

In one word, my contention is and was that this eminently justified caution should have expressed itself in the original telegrams. For the lack of it they misled all persons who were not capable of forming an independent opinion; *i. e.*, they misled everyone but the astronomers who comprehended the difficulty of reaching definite conclusions.

I have not criticised the Arequipa drawings because they are inartistic, but simply because they do not prove his assertions. Our own drawings are not artistic, and are not intended to be so. Whatever we announced in 1892, our published drawings prove. Professor PICKERING points out that I was mistaken in supposing that the shadows of *Jupiter's* satellites were not observed in their true shapes at Arequipa. I regret that I made this error, and I withdraw my remarks on this point. So far as I know, there are no published observations of the shapes of these shadows, except those of the LICK Observatory and the single one made in 1861 by Mr. BARNEBY.

With regard to the bright prominences observed at Arequipa on the limb of *Mars*, Professor PICKERING is in error in saying that "similar" observations had previously been made at the LICK Observatory. We observed no prominences at the full limb of the planet, but very many at the terminator, in 1890 and 1892. The observations at Arequipa and Mount Hamilton are by no means "similar."§

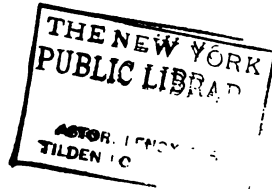
† *Astronomy and Astro-Physics*, 1892, page 675.

‡ *Op. cit.*, page 849.

|| *Op. cit.*, page 851.

§ See *Publications A. S. P.*, Vol. VI (1894), page 166.

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However our differences of opinion on this difficult subject may eventually be settled, our observations have already shown that Professor PICKERING's discoveries cannot be verified and nor have his conclusions of 1892 been confirmed at any observatory.

CHARTS OF FAINT STARS FOR MAGNITUDE COMPARISON. (SECOND SERIES.)

BY R. H. TUCKER, JR.

The series of charts V to VIII is in continuation of those in No. 32 of these *Publications*. Each begins at Right Ascension 2^m following some bright star, and extends 4^m, with a range of 10' in Declination, 5' each side of the bright star.

The charts as filled in at Harvard College Observatory with the 24-inch telescope, and further at Washington with the 26-inch, have generally been adopted without change, except for the addition in these charts of stars not seen here.

The stars are graded in five divisions, the highest, 8.9, including all stars brighter; the lowest grade, 16.17, is made up of stars newly introduced; but a number of the new stars have been put in the next higher grade, 14.15. The charts begin with the bright stars below:

		H.	M.			
I	follows α <i>Leonis</i>	10	2	+ 12 29	1893	
II	" η <i>Virginis</i>	12	14	+ 0 4	"	
III	" α <i>Bootis</i>	14	10	+ 19 44	"	
IV	" δ <i>Ophiuchi</i>	16	8	- 3 25	"	

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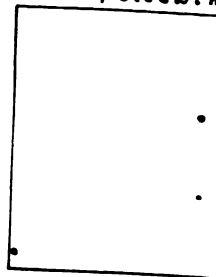
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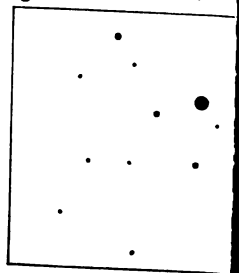
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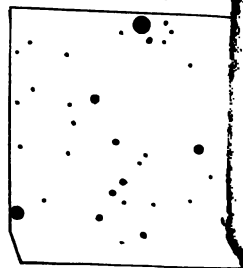
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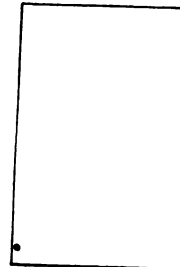
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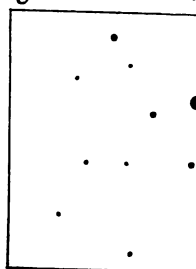
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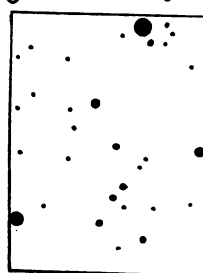
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VIII	" δ <i>Ophiuchi</i>	16	8	-	3 25	"

The number of stars in the charts before filling in with the

36-inch telescope, and the number of stars added, are as follows, omitting from the original number those not seen by me :

	Original.	New.	Total.
V	33	17	50
VI	23	8	31
VII	24	35	59
VIII	61	100	161

The stars of the completed charts fall in the following grades :

8.9	magnitude and brighter,	3
10.11	"	24
12.13	"	51
14.15	"	86
16.17	"	137

The observations were made with a power of 350, on the 36-inch telescope, giving a field of 5'.

LICK OBSERVATORY, July 20, 1894.

(SEVENTEENTH) AWARD OF THE DONOHUE COMET-MEDAL.

The Comet-Medal of the Astronomical Society of the Pacific has been awarded to Professor J. M. SCHAEERLE for his discovery of an unexpected comet on the negatives of the eclipse of April 16, 1893.

The Committee on the Comet-Medal,

EDWARD S. HOLDEN,
CHAS. BURCKHALTER.

July 24, 1894.

THE SPECTRUM OF MARS.

BY W. W. CAMPBELL.

The spectrum of *Mars* has been observed by several eminent astronomers — RUTHERFURD, SECCHI, JANSSEN, HUGGINS, VOGEL, MAUNDER. These observers had especially in mind the solution of two questions : (*a*) Is there spectroscopic evidence of an *atmosphere* on *Mars* ? and (*b*) Is there spectroscopic evidence of *aqueous vapor* in the atmosphere of *Mars* ?

The observations made in 1862 in America by RUTHERFURD* did not give an affirmative answer to these questions. The meagre results obtained seem to show that his instrumental equipment was not adapted to the solution of this problem.

The details of JANSSEN's observations have not been published, so far as I know, but his conclusions are expressed in a letter** read to the French Academy in 1867: "I cannot bring this letter to a close without telling you that I have ascended Mt. Etna for the purpose of making some spectroscopic observations which require a great altitude in order to annul the greater part of the influence of our atmosphere. From these observations and from those which I have made at the observatories of Paris, Marseilles, and Palermo, I believe I can announce to you the presence of aqueous vapor in the atmospheres of *Mars* and *Saturn*."

In 1867 HUGGINS† in England observed lines in the spectrum of *Mars* "apparently coincident with groups of lines which make their appearance when the Sun's light traverses the lower strata of the [Earth's] atmosphere, and which are therefore supposed to be produced by the absorption of gases or vapors existing in our atmosphere. The lines in the spectrum of *Mars* probably indicate the existence of similar matter in the planet's atmosphere."

We have not the dates and details of SECCHI's observations, but in 1872 he wrote‡ that he considered his observations "proved the existence of a Martian atmosphere analogous to our own." However, there appears to be a reasonable doubt as to the sufficiency of his observations.

The most extensive observations on this subject are those made by VOGEL in Germany in 1873. From his observations he considered that "it is definitely settled that *Mars* has an atmosphere whose composition does not differ appreciably from ours, and, especially, the Martian atmosphere must be rich in aqueous vapor."‡

Observations by MAUNDER‡‡ at Greenwich in 1877 confirmed in a general way those made by VOGEL.

* *American Journal Science*, Jan. 1863.

** *Comptes Rendus*, Vol. LXIV, page 1304.

† *Monthly Notices Roy. Ast. Soc.*, Vol. XXVII, page 179.

‡ *Sugli Spettri prismatici di Corpi celeste*, Rome, 1872.

‡ *Untersuchungen über die Spectra der Planeten*, page 20.

‡‡ *Monthly Notices Roy. Ast. Soc.*, Vol. XXXVIII, page 35.

It is seen that the investigations of five eminent spectroscopists lead them substantially to the same result, viz.: *That Mars' atmosphere is similar to our own.* Their conclusion has been very generally accepted by astronomers. A careful examination of all the published data has shown me that some of the observations were made under circumstances *extremely unfavorable*, and that between the different sets of observations there is not that close agreement which one would like to see. While I believed that *Mars* has an atmosphere and that it contains water vapor, it seemed to me that a repetition of the spectroscopic observations under the very favorable circumstances existing here would be valuable.

Among the favorable circumstances we may mention:

(1) Improved spectroscopic apparatus. The observations mentioned above were made from seventeen to thirty years ago, with spectroscopes comparatively crude.

(2) A telescope of great focal length and correspondingly large aperture. The telescopes used in the early observations were small and short, so that the images of *Mars* formed by them on the slit-plates would be less than one-third as large as that given by the 36-inch equatorial. This is an enormous advantage, both in estimating the relative intensities of spectral lines, and in comparing the intensities of the centres of the lines (corresponding to the centre of *Mars'* disc) with the intensities of the ends of the same lines (corresponding to the limb of *Mars*).

(3) The altitude of the observatory, which eliminates from the problem the absorptive effect of the lower 4200 feet of the Earth's atmosphere, with all its impurities. Most of the old observations were made from near sea-level.

(4) The very dry summer air prevailing here. The average relative humidity is very low at Mount Hamilton for the months of July and August. In many years it is less than 35 per cent. There is no difficulty in selecting nights for observing the spectrum of *Mars* when our relative humidity is not more than 20; quite frequently it is less than 20. This is a very important factor, since in examining *Mars'* spectrum for evidences of aqueous vapor it is very important, as JANSSEN pointed out in 1867, that we eliminate as far as possible the effect of aqueous vapor in our own atmosphere. The observers do not seem to have taken this factor into account (except JANSSEN, the details of whose observations appear to be unpublished). By examining

the contemporary weather data, I find that some of the observations were made when the relative humidity was 81, 85 and even 90. All the principal published observations were made where the average relative humidity at those seasons of the year is something like 80.

(5) The southern location of the observatory and the north declination of *Mars* permit the observations to be made when the planet's altitude is as great as 59° . At an altitude of 59° , the light from *Mars* passes through 1.17 times as much atmosphere as it would if the planet were in the zenith. The most important of the published observations were made when the planet's altitude was only from 21° to 26° . That is, its light passed through from 2.75 to 2.28 times as much of our atmosphere as it would had the planet been in the zenith! While the observers sought to eliminate the effect of our atmosphere and its aqueous vapor by observing the lunar spectrum when the Moon was at the same altitudes, it must be evident that the Martian spectrum was observed at a tremendous disadvantage. One observation was made, for instance, when the altitude of *Mars* was only 24° and the relative humidity of our own atmosphere was 85. The effects of any possible Martian atmosphere would be pretty thoroughly drowned by the effects of the great thickness of our own atmosphere, nearly saturated with moisture.

(6) Finally, we may mention that our knowledge of the spectrum of our own atmosphere has been largely increased in the last few years. THOLLON's excellent maps, for instance, are of great assistance in this problem.

With all these favorable circumstances, I expected that a confirmation of previous results would be a simple and easy matter.

We shall now state briefly the elements which enter into this problem.

We know by observation that the hemisphere of *Mars* which is turned toward the Sun is bright, and that the hemisphere which is opposite the Sun is dark. The planet, therefore, shines by reflected sunlight. The spectrum of *Mars* must be identical with that of the Sun, except as it is modified by the planet's (supposed) atmosphere.

The highly heated interior of the Sun, constituting its most dense portions, radiates light of all possible wave-lengths: that is, its spectrum is a strictly *continuous* band—not crossed by dark lines. The outer portions of the Sun are gaseous, of very much

lower temperature than the inner portions, and made up of the vapors of the chemical elements contained in the Sun. These vapors, mostly those of hydrogen and the metals, constitute a sort of *solar atmosphere*. The light radiated from the hotter interior of the Sun does not pass freely through this surrounding atmosphere. It absorbs some of the rays of every wave-length (but more especially the blue and violet rays). This is called a *general absorption*. It also selects light of *particular* wave-lengths and absorbs that light very strongly, producing the dark lines. The absorption which produces the dark lines is called *selective*, and the lines are called *metallic* lines. The solar spectrum consists of the *continuous spectrum* of the *Sun's interior* modified or interrupted by thousands of (dark) *metallic lines* caused by the *solar atmosphere*.

Our own atmosphere modifies the solar light which passes through it. It exercises both a *general* absorption, which weakens the continuous spectrum, and a *selective* absorption, which introduces at least 1200 additional dark lines. These dark lines—called *telluric* lines—constitute what we may term the spectrum of our atmosphere.

If the planet *Mars* is surrounded by an atmosphere, it no doubt exercises an absorption upon the solar light which enters it. The rays of light coming to us from the planet, originate in the Sun; they pass once through the solar atmosphere; they enter *Mars'* atmosphere, are reflected partly by the planet's surface and partly by the inner strata of its atmosphere, and then pass out through its atmosphere; and they finally reach us by passing once through our atmosphere. The spectrum of *Mars* is, therefore, the combined spectrum of the solar, Martian and terrestrial atmospheres. If it *has* no appreciable atmosphere, the spectrum of the planet is simply the combined spectrum of the solar and terrestrial atmospheres.

The problem before us would be practically insoluble if we did not have a convenient means of eliminating the solar and terrestrial spectra, and leaving only *the Martian spectrum*. Our Moon has ~~no~~ appreciable atmosphere. Consequently, its spectrum is the combined solar and terrestrial spectrum. If we compare the spectra of *Mars* and the Moon when these bodies are at the same altitude above our horizon,—that is when their light traverses the same thickness of terrestrial atmosphere,—and find that they differ in any respect, however slight, such difference must be

caused by an atmosphere on *Mars*. If no difference is found to exist, then the spectroscope affords no evidence of such an atmosphere. Thus, the problem resolves itself into a comparison of the Martian and lunar spectra.

THOLLON has found that in the combined solar and terrestrial spectrum three very strong groups of lines are produced by some of the *constant* elements of our atmosphere, probably by the oxygen. These are the FRAUNHOFER groups A, B and α , comprising about 130 separate lines. The presence of these lines indicates the presence of *atmosphere*. If they are *stronger* in the Martian spectrum than in the lunar spectrum, that planet must have an atmosphere.

THOLLON* found other groups of lines, comprising at least 1100 separate lines, produced by the *aqueous vapor* in our air. They have been divided by THOLLON into the following seven groups :

- | | | | |
|-----|--------------|------------|--------------------------|
| (1) | Wave-lengths | 745 to 716 | (FRAUNHOFER's α) |
| (2) | " " | 716 " 687 | (below B) |
| (3) | " " | 660 " 646 | (around H α) |
| (4) | " " | 635 " 628 | (near α) |
| (5) | " " | 597 " 585 | (around D) |
| (6) | " " | 578 " 567 | (BREWSTER's δ) |
| (7) | " " | 548 " 542 | |

The presence of these groups of lines indicates the presence of *aqueous vapor*. If they are *stronger* in the Martian spectrum than in the lunar spectrum, there is *aqueous vapor* in the atmosphere of *Mars*.

Now while all these lines can be observed *individually* in the solar spectrum, owing to the high dispersion which can be used, they can only be observed as groups or bands in the Martian and lunar spectra, on account of the faintness of those spectra and the low dispersion which must be employed.

It is impracticable to observe the groups A, 745-716 and 716-687, which are at the extreme red end of the spectrum, and they will not be further considered. The atmospheric bands B and α are easy to observe in both spectra. The vapor groups* of lines require great care in observing, for the reason

* It must not be considered that the credit of this work is due wholly to THOLLON. Many observers, BREWSTER, GLADSTONE, JANSSEN and others, investigated along the same lines. But THOLLON's work is most complete and his maps are especially convenient and useful.

that, owing to the low dispersion which must be used, the individual lines are not only blended with each other but also with the solar *metallic* lines which lie among them. In the 7th group, for instance, the vapor lines are so much fainter than the neighboring metallic lines that we need not consider that band in the present problem. For the same reasons the 6th group, 578-567, is not a sufficiently sensitive test for aqueous vapor, except in the Earth's atmosphere when the body observed is near the horizon. However, the region of the 6th group was carefully observed in the Martian and lunar spectra on several nights. The 4th group, 635-628, is useless as a test for aqueous vapor, since the faint lines composing it are always overwhelmed by the prominent lines in the atmospheric group *a*. Only the 3d and 5th groups remain available. Of the 3d group I have not found useful that portion of it between 660 and 653, on account of the presence of the very heavy *H α* solar lines and other solar lines among the relatively faint vapor lines. I have for my own use divided the rest of the 3d group into three parts, each of which was found useful. The first covers wave-lengths 6515-20 and includes about eight tolerably strong lines, the majority of which are vapor lines. Under all of the dispersions used it was simply a very narrow band or line, which I shall call *c'*. The second part covers the region 6491-6500. It includes half-a-dozen strong metallic lines and a few strong vapor lines, all, however, blending to form a very strong, narrow band or line, which we shall call *c''*. The third part is included between 6463 and 6490, which contains a great many vapor lines and a few metallic lines. It formed a band of good width which we shall call *c'''*.

The 5th group, extending from 597 to 585, I divided into four parts. The first covers wave-lengths 5941-5959; it contains a number of strong aqueous lines and several metallic lines, forming a band which I called *d'*. The second covers 5928-35; it is strong in neither metallic nor vapor lines. It forms a narrow band which I called *d''*. The third portion covers 5912-25; it contains a few metallic lines and very many strong vapor lines; I called this region *d'''*. The fourth covers 5884-5906; it contains the two very strong solar lines *D₁* and *D₂*, several faint solar lines, and a great many vapor lines. It would be a very useful band if the *D* lines were not contained in it; but I found their presence very troublesome. Let us call this region *d^{iv}*.

For the reasons given above I confined my observations almost wholly to the groups B, α , c' , c'' , c''' , d' , d'' , d''' and d^{iv} . Of these, I found that α , c' , c''' , d' , d''' were best suited for observation.

I observed the spectrum of *Mars* on ten nights between June 29 and August 10 of the present year, paying special attention to the nine critical groups of lines just mentioned. On eight of the nights I compared its spectrum with that of the Moon, when these two bodies were at equal distances above the horizon. On two nights, July 24 and 25, when the Moon was near the planet, I turned repeatedly from one spectrum to the other, while, on the former night, the planet passed from altitude 18° to 50° , and on the latter night, from altitude 45° to 55° . The two spectra have been compared when the relative humidity of our atmosphere was only 15 and when it was as high as 55. The observations were made principally with a dense 60° flint prism, with magnifying powers of 13 and 7, and occasionally with a 30° prism and power 13. When the lunar spectrum was examined, the slit of the spectroscope was always shortened so that the lunar spectrum was the same width as the Martian spectrum. The slit was directed always upon the brightest region of the Moon in order that the two spectra should be nearly of the same brightness, which is a very important condition in making reliable comparisons. In a word, the spectra have been compared under a variety of conditions, but with the conditions for the two bodies always identical. The atmospheric and aqueous vapor lines have been seen in both *Mars* and the Moon, decreasing in intensity as these objects got higher and higher in the sky, and the aqueous vapor lines varying in intensity with the amount of moisture in our atmosphere. At all times the spectrum of *Mars* has appeared to be identical with that of the Moon in every respect.

Further, on several occasions when the planet's altitude was large, I examined the critical groups of lines, especially α , to determine whether the ends of the lines, which correspond to the limb of the planet, were stronger than their centres, which correspond to the centre of the disc. The lines appeared to be of uniform intensity throughout, so far as the different intensities of different portions of the surface permitted a safe estimate to be made.

The intensity of the critical bands, α for instance, was appreciably greater when the Moon and *Mars* were only 30° above the horizon than when they were 55° . The relative thicknesses of

our atmosphere traversed by the rays when the bodies were at altitudes of 30° and 55° were as 2 to 1.22. If the rays of light from one of the bodies, *Mars* for instance, pass through a unit thickness of our atmosphere, and the rays from the Moon pass through $1\frac{1}{2}$ units, the intensity of α in the spectrum of the latter is certainly greater than in the spectrum of the former. In fact, I am quite confident that a difference of 25 per cent. in the lengths of paths traversed by the rays from the two bodies would cause an appreciable difference in the intensities of their α bands. The accuracy of the observation is greatly increased by the presence of several neighboring metallic lines, which can be used as standards of comparison.

The results of these observations are as follows :

First.—The spectra of *Mars* and our Moon, observed under favorable and identical circumstances, seem to be identical in every respect. The *atmospheric* and *aqueous vapor* bands which were observed in both spectra appear to be produced wholly by the elements of the Earth's atmosphere. The observations, therefore, furnish no evidence whatever of a Martian atmosphere containing aqueous vapor.

Second.—The observations do not prove that *Mars* has *no* atmosphere similar to our own; but they set a superior limit to the *extent* of such an atmosphere. Sunlight, coming to us *via Mars* would pass twice either partially or completely through his atmosphere. If an increase of 25 to 50 per cent. in the thickness of our own atmosphere produces an appreciable effect, a possible Martian atmosphere one-fourth as extensive as our own ought to be detected by the method employed.

Third.—If *Mars* has an atmosphere of appreciable extent, its absorptive effect should be noticeable *especially* at the limb of the planet. My observations do not show an increased absorption at the limb. This portion of the investigation greatly strengthens the view that *Mars* does not have an *extensive* atmosphere.

While I believe that the polar caps on *Mars* are conclusive evidence of an atmosphere and aqueous vapor, I do not consider that they exist in sufficient quantity to be detected by the spectroscope. This view has an important bearing upon the questions relating to the low albedo of the planet, and the well-known brightness of its limb, in both of which respects the planet resembles our Moon.

MOUNT HAMILTON, 1894, August 14.





LICK OBSERVATORY, FROM THE NORTHEAST.

(By the courtesy of *McClure's Magazine*.)



NOTICES FROM THE LICK OBSERVATORY.

PREPARED BY MEMBERS OF THE STAFF.

II. DISCOVERY OF COMET I, 1893.

By the kindness of Professor EDWARD C. PICKERING, the LICK Observatory has just received a fine set of lantern-slide copies of the negatives of the eclipse of April 16, 1893, which were made by the Harvard College Observatory Eclipse Expedition to Chile, under the direction of Professor WILLIAM H. PICKERING.

The coronal comet first discovered by Professor J. M. SCHAEFERLE on the LICK Observatory negatives* is unmistakable on the copies of the Harvard College Observatory plates, Nos. 454, 467 and 468. It is also visible on the plates of the British expeditions in Brazil and in Africa.

The form of the comet, so far as shown on the Harvard College Observatory plates, is practically the same as the form exhibited by the negatives of the LICK Observatory. The position is substantially the same on both sets of plates, as the difference in time between the two stations in Chile was but a fraction of a minute.

E. S. H.

LICK OBSERVATORY, July 23, 1894.

THE CORONAL COMET OF APRIL 16, 1893.

The LICK Observatory has just secured copies of the eclipse-photographs taken by Professor W. H. PICKERING in Chile on April 16, 1893. The coronal comet is unmistakably shown on at least three of the plates (Nos. 454, 467 and 468), having practically the same form and position as in the LICK Observatory

* See *Publications A. S. P.*, Vol. VI (1894), page 144.

photographs; the difference in the times of exposure at the two stations being only a fractional part of a minute.

The comet was discovered on the LICK Observatory plates, and subsequently copies of the negatives of the British expeditions to Brazil and Africa were received at the LICK Observatory and the comet was found on these plates also, the cometary character of the object being firmly established by the motion. (See *Astronomical Journal*, No. 318).

I learn from the *Observatory* for July, 1894 (page 254), received July 25, that the originals from which these copies were made have been examined in London, and that the comet has not been seen. It is true that the British negatives of the eclipse are not so rich in detail nor so strong in contrasts as our own, but there is no question that the comet is visible on them also. If the observers have not seen it after having their attention directed to it, it can only be because they have not properly examined the negatives. I will undertake to show this comet to anyone on at least ten LICK Observatory negatives; on at least three Harvard College copies, and on at least four of the British copies unmistakably. I have just made and sent to the Royal Astronomical Society a negative copy of one of the British positives and indicated the exact position by a series of arrows. As the object is now shown in a very conspicuous manner it cannot fail to be seen at once.

J. M. S.

LICK OBSERVATORY, July 26, 1894.

VIEWS OF THE EYE-END OF THE LARGE TELESCOPE, AND OF THE LICK OBSERVATORY.

The engravings of the eye-end of the 36-inch equatorial, and of a view of the LICK Observatory from the northeast, are printed from electrotypes kindly presented to the LICK Observatory by Mr. S. S. McCCLURE. The originals are to be found in the August number of *McClure's Magazine*. E. S. H.

LIFE OF MADAME SOPHIE KOVALEVSKY; PROFESSOR OF
HIGHER ANALYSIS IN THE UNIVERSITY OF STOCKHOLM.

Those who have not read the *Souvenirs de Sophie Kovalevsky*, by her friend Madame EDGREN-LEFFLER, will be glad to be referred to an article in the *Revue des Deux Mondes* (May 15, 1894) under the title "La Rançon de la Gloire," which gives a sketch of the private, as well as of the public, life of this woman of genius.

Her memoirs have been printed, at her own request, in order that those who knew only the outer facts of her career and its immense successes in the field of pure mathematics, might be able to judge her life in its totality. Her own final verdict was : "Les travaux scientifiques ne donnent pas la joie, et ne font pas avancer l'humanité. C'est folie d'y perdre sa jeunesse; c'est un vrai malheur que d'avoir le don des sciences, en particulier pour une femme, qui est alors poussée de force dans une sphère d'activité où elle ne peut pas trouver le bonheur."

Her memoirs appeal to a wider public than that which wishes to follow the development of the mathematical talent of a great mind. The conclusion of her biographer is well expressed in the words of another woman of genius: * "La gloire, pour une femme, n'est jamais que le deuil éclatant du bonheur."

E. S. H.

MERIDIAN CIRCLE OBSERVATIONS AT THE LICK OBSERVATORY, JULY, 1893, TO JULY, 1894.

The meridian circle has been employed during the past year mainly in the observation of a list of 125 stars selected by Professor HOLDEN from the *American Ephemeris, Connaissance de Temps*, and *Nautical Almanac*. These are stars not included in the Berlin *Jahrbuch*, and are to be reduced with stars from the last as standard.

Four observations have been made of each star in one position of the instrument, and will be repeated in the reversed position.

Six hundred observations have been made of the stars of the B. J. selected by Professor AUWERS, in the belt from -10° to $+15^{\circ}$ Declination to serve as fundamentals, and 450 observations of the circumpolar stars of the same list.

* Madame de Staël.

Joie

S/
A/

A determination of the collimation correction is made for each night's work, and three of the zenith point and level, by nadir.

The places of various short lists of comparison stars have been determined; and with the observations made for special investigations, and for clock corrections on a few nights other than those used for regular observing, 2300 observations have been made during the year.

R. H. T.

July 27, 1894.

FOREST FIRES AT MOUNT HAMILTON, JULY 29 TO
AUGUST 3, 1894.

The forest fires of 1891 have been spoken of in these *Publications*, Vol. III, page 292, and Vol. V, page 109. A poor reproduction of a good photograph of the great fire on Mount Santa Ysabel is printed in Vol. V, facing page 61. These fires burned around a semi-circle from north (through east) to south; they were never nearer than 4000 feet to the Observatory; and they were confined by our work or otherwise to the further slopes of the mountains round about.

Some time on July 28 or 29, 1894, a fire started in a deep cañon to the west of the Observatory and to the north of the brick-yard. Its smoke was very obvious all day, Sunday, July 29. On Monday the Observatory workmen, with one volunteer, cut a narrow trail from a point above the fire down to the trout stream in the bottom of Cañon Negro, which lies north and northeast of the Observatory peak. This trail was between the forest fire and the Observatory, and beyond the trail back fires were set to widen the line of protection. On Tuesday morning Mr. MACDONALD, machinist of the LICK Observatory, went twice over this line from its beginning to the trout stream (some 1500 feet below the summit) and found no place in which the forest fire had crossed our back fires. At noon on Tuesday, however, the fire had crossed the line, and was making great progress towards the Observatory, and especially towards the wooden cottages near the Observatory and the wooden stables and sheds. Fortunately, a considerable space below these buildings along the road to the springs—Aquarius—had been cleared of trees and brush in 1891. This space was once more quickly cleared off and a line of back fires set along the Aquarius road. More men were sent for, and

during Tuesday and Wednesday (day and night) there were some forty persons employed in fighting the fire (including all the astronomers of the LICK Observatory). About two miles of back fires were set and controlled. No damage whatever was done to any of the wooden cottages, to the stables, to the house of the United States Coast and Geodetic Survey, to the water tanks, to the engine and pump houses at the springs. These were protected by our labor. The buildings of the LICK Observatory were certainly at no time in danger. It is probable that we rather under-estimate than over-estimate the risk from such fires as these. Unless the winds are very high* (say 30 miles per hour and upwards), it seems easy to control a forest fire of which one has had sufficient warning. With high winds carrying large sparks it is almost impossible to defend long lines or large areas. In such cases the wise course is to defend buildings and their immediate surroundings and to let the rest go. At the worst, we only lose a few hundred acres of the *chapparal*, which renews itself within a few years. In one respect the present fire was vastly easier to manage than those of 1891. It was so near to the buildings that the water supply of the LICK Observatory could be and was utilized. After using all that was necessary there remained: In COPERNICUS reservoir, 15,000 gallons; in KEPLER reservoir, 122,000 gallons; in HUYGHENS' reservoir about 50,000 gallons, and a good supply at the springs.

Several photographs were taken by the astronomers in the intervals of their service at the fire, and it is hoped to reproduce one or more of them here. We become so used to the changed conditions of life at Mount Hamilton that it does not strike us as strange to see members of the University faculty in the rôle of firemen, not to say salamanders. To realize the incongruity it is necessary to shift the scene and to imagine the fire to be in Greenwich Park or at Enckeplatz, S. W., Berlin!

Efficiency is a virtue, wherever it is found, and the astronomers of the LICK Observatory may justly claim to be efficient in the theory and practice of fighting forest fires.

E. S. H.

LICK OBSERVATORY, 1894, August 4.

* The highest wind velocity of Tuesday was 15 miles per hour at 11 P. M.; the highest of Wednesday was 20 miles from 3 to 8 A. M. and from 5 P. M. to 12 P. M.

VARIATION OF LATITUDE IN SAN FRANCISCO, AS DETERMINED BY PROFESSOR GEORGE DAVIDSON.

The *Astronomical Journal*, No. 323, contains an account of a very extensive and important series of observations made by Professor GEORGE DAVIDSON, between May 27, 1891, and August 19, 1892, to determine the variation of the latitude of the place of observation in San Francisco. Two instruments were employed in the work. The probable error of a single determination of latitude with the first instrument was 0".28. The second instrument gave more accurate results, the error being 0".19. The corresponding errors for Rockville and Honolulu, where similar instruments were employed, were 0".17 and 0".21 respectively. The less accurate results from the first instrument are probably due to the defective stability of the old form of coast survey zenith telescope and perhaps to the inferiority of its levels, and to the fact that only one level was employed for part of the time. Whatever was lacking in instrumental equipment was more than made up, however, by the very large number of observations, no less than 6768 determinations of the latitude having been secured on 237 nights.

The minimum value of the latitude (observed) was

$$1891, \text{ October } 22, \phi = 37^{\circ} 47' 28''.118.$$

The maximum value was

$$1892, \text{ May } 15, \phi = 37^{\circ} 47' 28''.517.$$

The range was then about 0".4, corresponding to about 40 feet on the Earth's surface.

Professor DAVIDSON's observations are well represented by the following formula (where t is the number of days after January 0, 1891):

$$\begin{aligned} \phi = 37^{\circ} 47' 28''.334 + 0''.171 \sin(0.83526 t + 3^{\circ}.7) \\ + 0''.074 \sin(0.98563 t + 20^{\circ}.5). \end{aligned}$$

The periods of the two cyclic functions were assumed from CHANDLER'S values, namely, 431 and 365.25 days respectively; and, as has been said, the observations are well represented on that assumption. It is understood that Professor DAVIDSON is continuing this important research.

E. S. H.

LICK OBSERVATORY, 1894, August 4.

SUGGESTIONS TO MEMBERS OF THE ASTRONOMICAL SOCIETY
OF THE PACIFIC.

Professor MALCOLM MCNEILL, of Lake Forest University, has kindly consented to prepare for the Society notes of planetary and stellar phenomena for each month, for the convenience of our members. The notes for November and December, 1894, are printed in the present number of the *Publications*. These notes will be interesting and valuable not only to our members but to a wider circle.

It is suggested, therefore, that each member should see that the newspapers in his vicinity reprint these notes of Professor MCNEILL's about the first of the months to which they refer. By taking a little pains to spread this information our members may render a useful service to their respective communities.

E. S. H.

LICK OBSERVATORY, August 1, 1894.

REPRODUCTION OF A PHOTOGRAPH OF COMET *b*, 1893.

An excellent half-tone reproduction of a photograph of Comet *b*, 1893, RORDAME, has been published in the July (1894) number of *Knowledge*. This photograph was taken with the CROCKER telescope of the LICK Observatory, July 13, 1893. No mention of this is made in connection with the reproduction. In these *Publications*, Vol. V, page 143, and in *Astronomy and Astro-Physics*, Vol. XII, page 661, I have given a brief account of this photograph and also of some of the other photographs which I obtained of this comet.

W. J. HUSSEY.

STANFORD UNIVERSITY, CAL., August 6, 1894.

REQUEST FOR OBSERVATIONS OF THE METEOR OF JULY 27,
1894.

The LICK Observatory will be glad to receive any observations of the meteor of July 27, 1894, particularly such as will yield accurate values of the *altitude*, the *azimuth* and the times of appearance, explosion and disappearance of this body. The observations now in hand indicate that the meteor exploded some thirty miles above a point a dozen miles or so from Merced, while it was falling at a large angle to the Earth's surface. The meteor was 80½ miles from the LICK Observatory when it burst.

E. S. H.

LICK OBSERVATORY, August 4, 1894.

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NOTICE.

The attention of new members is called to Article VIII of the By-Laws, which provides that the annual subscription, paid on election, covers the *calendar* year only. Subsequent annual payments are due on January 1st of each succeeding calendar year. This rule is necessary in order to make our book-keeping as simple as possible. Dues sent by mail should be directed to Astronomical Society of the Pacific 819 Market Street, San Francisco.

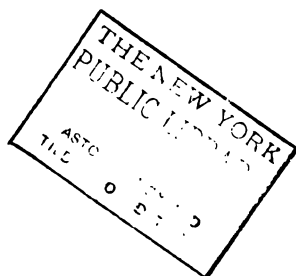
It is intended that each member of the Society shall receive a copy of each one of the *Publications* for the year in which he was elected to membership and for all subsequent years. If there have been (unfortunately) any omissions in this matter, it is requested that the Secretaries be at once notified, in order that the missing numbers may be supplied. Members are requested to preserve the copies of the *Publications* of the Society as sent to them. Once each year a title-page and contents of the preceding numbers will also be sent to the members, who can then bind the numbers together into a volume. Complete volumes for past years will also be supplied, to members only, so far as the stock in hand is sufficient, on the payment of two dollars to either of the Secretaries. Any non-resident member within the United States can obtain books from the Society's library by sending his library card with ten cents in stamps to the Secretary A. S. P., 819 Market Street, San Francisco, who will return the book and the card.

The Committee on Publication desires to say that the order in which papers are printed in the *Publications* is decided simply by convenience. In a general way, those papers are printed first which are earliest accepted for publication. It is not possible to send proof sheets of papers to be printed to authors whose residence is not within the United States. The responsibility for the views expressed in the papers printed rests with the writers, and is not assumed by the Society itself.

The titles of papers for reading should be communicated to either of the Secretaries as early as possible, as well as any changes in addresses. The Secretary in San Francisco will send to any member of the Society suitable stationery stamped with the seal of the Society, at cost price, as follows: a block of letter paper, 40 cents; of note paper, 25 cents; a package of envelopes, 25 cents. These prices include postage, and should be remitted by money-order or in U. S. postage stamps. The sendings are at the risk of the member.

Those members who propose to attend any or all of the meetings at Mount Hamilton during the summer should communicate with "The Secretary Astronomical Society of the Pacific" at the rooms of the Society, 819 Market Street, San Francisco, in order that arrangements may be made for transportation, lodging, etc.







PUBLICATIONS
OF THE
Astronomical Society of the Pacific.

VOL. VI. SAN FRANCISCO, CALIFORNIA, DECEMBER 1, 1894. No. 38.

THE LICK OBSERVATORY ECLIPSE EXPEDITIONS
OF JANUARY, 1889, DECEMBER, 1889,
AND OF APRIL, 1893.

BY EDWARD S. HOLDEN.

The LICK Observatory is about to print Professor SCHAE-BERLE's report of his observations of the total solar eclipse of 1893 in Chile. This is, then, a favorable time for giving in one place, and in brief, the chief results obtained by the three eclipse expeditions sent out from the LICK Observatory.

When the LICK Observatory was opened for work in June, 1888, it was fully provided with fixed instruments, but its photographic equipment consisted of small cameras only.

The total solar eclipse of January 1, 1889, which was visible in California, had to be observed with such apparatus as was then available; and it was not practicable to obtain photographic cameras of large aperture and relatively short focus at that time. It was therefore determined to make the chief work of the LICK Observatory party in the field (at Bartlett Springs) spectroscopic in character. At the eclipse of May, 1883, Professor HASTINGS (of Yale University), observing with a special spectroscopic apparatus, devised by himself, had compared the spectra of the opposite limbs of the Sun, and had come to the conclusion that the solar corona was, in great part, a diffraction phenomenon, and therefore that the corona had no objective existence.*

As Professor HASTINGS' conclusions had not been generally accepted, and as his observations had never been verified or

* *Memoirs of the National Academy of Sciences*, Washington, Vol. II, 1883, page 102, *et seq.*

disproved, it was arranged that Professor KEELER, chief of the LICK Observatory party, should employ a duplicate of the apparatus devised by Professor HASTINGS, and should repeat the observations made at the 1883 eclipse.

The results of Professor KEELER are given in the Report on the Observations of the Total Eclipse of the Sun of January 1, 1889, page 31 *et seq.* His deductions from the spectroscopic observations alone are (*op. cit.* page 49): "We must conclude that the coronal outlines and details are altogether independent of the Moon's position on the Sun, and their objective existence is rendered extremely probable. It would be reduced to a certainty by the undoubted observation of the limb of the Moon nearest the Sun when entirely clear of his disc, since the Moon could only be visible under these circumstances by contrast with a bright background on which it was projected, in which diffraction could have no part." Precisely this observation was recorded on two negatives taken by Mr. E. L. WOODS, an amateur photographer, *before the beginning of totality*.* These negatives are now at the LICK Observatory.

The observations of Professor KEELER, in connection with those of Mr. WOODS, conclusively decided the question proposed for solution. The corona is not, in the main, a phenomenon due to diffraction; it has an objective existence.†

The spectroscopic observations made by Professor KEELER also established the very important fact that the observed length of a coronal line at an eclipse is no index of the depth of the corresponding gas in the solar surroundings.‡

We may then consider the Sun to be surrounded by comparatively shallow envelopes, and great theoretical difficulties are thus removed; as, for example, questions connected with the unretarded passage of comets very close to the Sun's surface.

The meagre photographic equipment of the Observatory did not promise the excellent results which were, in fact, obtained in this department. These results were, in great part, due to the instructions to photographers distributed from the Observatory, which were followed by our own party and by many amateurs. The instructions strongly recommended very short exposure-

* See the Eclipse Report of January 1, 1889, page 164, etc.

† Visual observations of the phenomenon photographed by Mr. Woods are reported by General C. W. IRISH. See the Eclipse Report of January, 1889, page 191.

‡ See the Report, page 47.

times,* and they were based on experiments made here by Professor S. W. BURNHAM on photographing faint contrasts on a bright background (light wisps of cloud near the over-clouded Sun).

The Amateur Photographic Association of the Pacific Coast volunteered to send a large party of observers to photograph the eclipse under the direction of Mr. CHARLES BURCKHALTER of the CHABOT Observatory of Oakland. This party carried out a programme which had been previously arranged with Mr. BURCKHALTER at Mount Hamilton. The large number of observers allowed experiments in photographing the corona to take a very wide range, in respect of the kind of plates employed, of the light ratio of the cameras, and of the exposure-times. The very valuable observations of these gentlemen are printed in the Eclipse Report of January, 1889, and most of the negatives made by them were generously presented to the LICK Observatory, and are preserved in its collections. I think there is no case on record of so general and so intelligent a seconding of scientific plans by a society of amateurs. The photographic observations of the eclipse of January, 1889, by our own party and by the Amateur Photographic Association, have afforded a sure basis for all subsequent photographic campaigns undertaken by the LICK Observatory eclipse expeditions. The formation of the Astronomical Society of the Pacific is a direct outcome of the eclipse expeditions of January, 1889.

The photographic work of the LICK Observatory expedition of January, 1889, was done by Professor E. E. BARNARD with small cameras, and with a camera improvised by him from a small portable telescope of $3\frac{1}{4}$ inches aperture and 48.90 inches photographic focus. By stopping down the latter instrument to an aperture of 1.75 inches, and by taking extreme pains in focusing, and in all the subsequent photographic manipulations, Professor BARNARD obtained negatives of the corona which, notwithstanding their comparatively small size, have no superiors among eclipse photographs up to that date.

The methods of photographic photometry, which were first proposed by Captain ABNEY, F.R.S., and which were in use at the HARVARD College Observatory, were adopted at this eclipse, and have been adhered to at subsequent eclipses observed by parties

* Suggestions for Observing the Total Eclipse of the Sun, of January 1, 1889, page 18.

from the LICK Observatory. The resulting plates have been studied by Professor LEUSCHNER, and their discussion is to form a part of his thesis for the degree of Ph.D.

The photographic photometric measures of this eclipse (and of subsequent eclipses observed by LICK Observatory parties) are provisionally reduced in the Reports, under the assumption that the photographic action on the plate varies directly as the product of the intensity of the light and the time of exposure.

This assumption rested on the authority of Captain ABNEY. Our own trials soon showed that whatever the true law might be, this law was incorrect, and Captain ABNEY's most recent work does not confirm his earlier conclusions.*

It is very possible that no data of great value can be obtained from comparisons of this kind between the effects of two sources of light of *different spectra* on sensitive films exposed for *different times*. The observations will, however, be continued on the same plan at future eclipses until this point is established.

The negatives of the January eclipse were on a very small scale, and required to be magnified in order to exhibit the manifold detail contained in them. An enlarged drawing made by me from the negatives of Professor BARNARD contains no less than 113 reference numbers to separate features. Besides the detail in the immediate vicinity of the Sun, these negatives showed a very noteworthy trumpet-shaped extension, which can be followed to 77' from the Sun's centre. This extension is shown on a negative which was exposed for four and one-half seconds, during which time the photographic spreading, or diffusion, of the image in the film would be very small. The negatives secured by the U. S. Naval Observatory expedition under my charge in Colorado (July 29, 1878) were exposed about twenty seconds and showed an extension of 50' from the Moon's limb; those of the British and French expeditions to Caroline Island (May, 1883) were exposed for two and five minutes, respectively, and show extensions of about 60' from the limb.

At the California eclipse this extension was excellently shown on negatives made by the parties sent from the LICK Observatory, from the HARVARD College Observatory, and from the WASHINGTON University of St. Louis, as well as on many of the negatives secured by the members of the Amateur Photographic

* *Publications A. S. P.*, Vol. VI, 1894, page 67.

Association of the Pacific Coast. Among the latter negatives those of Messrs. IRELAND and LOWDEN were especially successful, as they showed the coronal extensions out as far as 135' and even 165', with exposures of 10 (21 and 41) seconds. The reality of this extension has been doubted by various astronomers. It was, however, photographed by Professor SCHAEBERLE at the eclipse of April, 1893, where it extends 190' east and 246' west of the Sun's center, with an exposure of four seconds. It must therefore be accepted as a real feature. If the Mechanical Theory of the Corona proposed by Professor SCHAEBERLE be correct, the existence of such an extension is called for by theoretical considerations. In any event, it has a real existence.

The enlarged drawing made from Professor BARNARD's negatives also brought out in clear relief the fact that the so-called "polar rays" were by no means confined to the poles, but extended all round the disc of the Sun, in the equatorial as well as the polar regions. The excellent definition of the negatives permitted such rays to be traced (for the first time, so far as I know) all through the bright "wings" at the equator. This new result of observation required a new theory to account for it. Its interpretation follows directly from the Mechanical Theory of Professor SCHAEBERLE.

The Report of the Eclipse of January, 1889, contains a number of observations of interest and importance communicated by volunteer observers. Among these, I may point out Dr. LEWIS SWIFT's observations of *white* prominences around the Sun (*op. cit.*, page 203), thus confirming the results of TACCHINI.* The same phenomenon may have been observed (in part) by General IRISH (*op. cit.*, page 193, line 13, from bottom).

The results derived from the observations of the eclipse of January 1, 1889, made it especially desirable to take advantage of the experience then gained, and to observe the total solar eclipse of December 21, which was to be visible in French Guiana. It was, however, found to be impossible to fit out an expedition to this distant point from the funds annually allotted to the LICK Observatory, and the plan had been reluctantly given up, when Hon. CHARLES F. CROCKER, one of the Regents of the University, generously offered to bear the whole expense of an expedition to South America.

* *Atti d. Accad. dei Lincei*, 1889, page 472.

On account of the remoteness of the observing station, and the consequent expense, it was decided to make the expedition wholly photographic, and accordingly Professors BURNHAM and SCHAEBERLE were provided with photographic apparatus only. The results of the expedition are given in an octavo volume, issued by authority of the Regents of the University in 1891, viz: "Reports on the Observation of the Total Eclipse of the Sun, December 21-22, 1889," etc. The weather at the eclipse was not entirely favorable, but excellent negatives were secured by both observers with various instruments.

The most important outcome of this expedition is contained in Part III of the report above mentioned. It is A Mechanical Theory of the Solar Corona, by Professor SCHAEBERLE, whose statement of his conclusions is as follows (*op. cit.*, page 48):

"Three well-known facts serve as a basis on which the whole theory rests. They are :

"(1) *The eruptions of the Sun's surface are most active and numerous in the spot-zones.*

"(2) *The Sun rotates about an axis passing through its center.*

"(3) *This axis is inclined to the plane of the Earth's orbit at an angle of about $82\frac{3}{4}^{\circ}$.*

"As the present theory also enables one to reproduce, graphically, practically all the phenomena observed during a total eclipse of the Sun, and as the graphical construction is in strict accordance with known mechanical laws, the following brief statement of the theory may be properly made here:

"The theoretical corona is caused by light emitted and reflected from streams of matter ejected from the Sun, by forces, which, in general, act along lines normal to the surface of the Sun; these forces are most active near the center of each Sun-spot zone.

"Owing to the rotation of the Sun, the streams of matter will not lie along normals. since the angular velocity of different portions of a stream grows less as the distance from the Sun increases; in other words, the streams are of double curvature. Each individual particle of the stream, however, describes a portion of a conic section, which is a very elongated ellipse, so long as the initial velocity is less than 383 miles per second (assuming that the Sun's atmosphere, as shown by various observations, is exceedingly rare).

"Certain variations in the type of the corona admit of an exceedingly simple explanation, being principally due to the change in the position of the observer with reference to the plane of the Sun's equator. *According as the observer is above, below, or in the plane of the Sun's equator, the perspective overlapping and interlacing of the streamers cause the observed apparent variations in the type of the corona.*

"The general direction in which these ejective forces act will be along radii. There may, of course, be numerous particular exceptions to this general direction.

"Similarly, since observation shows that certain solar motions are apparently confined to special regions, which are symmetrically situated with reference to the axis of rotation of the Sun, we are evidently justified in assuming that the forces which cause these motions are, in general, uniformly distributed around each circle of latitude, since there is no *a priori* reason why, in a highly heated rotating mass of matter, the solar activity should be confined to any particular circles of longitude. There may, of course, be particular exceptions to a uniform distribution of the forces.

"As the exact nature of the atmosphere surrounding the Sun is unknown, and as its extreme rarity has been demonstrated by various observations, the heliocentric motions of all particles exterior to the Sun's visible surface will be considered to be unimpeded by solar atmospheric resistances."

The theory announced in the general terms just given was put into a mathematical form by its author and was subsequently compared with, and found to agree with, the results of observation at all previous eclipses, including those which had been photographed by LICK Observatory parties.

A very favorable opportunity for testing this mechanical theory of the corona presented itself in the total solar eclipse of April 16, 1893, which was visible in Chile. By the aid of a timely grant of money from Mrs. PHOEBE HEARST, of San Francisco, it was possible to provide for the expenses of an expedition from the LICK Observatory under the charge of Professor SCHAEBERLE. The services of several volunteer aids were secured by him, and the expedition, which was wholly photographic, was eminently successful in every respect.

The plans for the photographic work were made by Professor SCHAEBERLE, in particular those for utilizing the 40-foot photo-

graphic objective of the photo-heliograph. The magnificent large-scale photographs (on 18 x 22 plates) thus secured have opened a new era in eclipse photography. They permit very small features of the corona and of the prominences to be represented on a scale sufficiently large to make them intelligible. Some of these features also exist in the enlarged *drawings* made by Mr. WESLEY from the negatives of the eclipse of 1871; but while they were (partly) represented, they could not then be completely interpreted.

It is hoped to secure similar large-scale photographs on the same plan and with the same instrument at the total solar eclipse of 1896 in Japan. At the same eclipse, it is also hoped to renew the spectroscopic investigations of the corona, which have been neglected since January, 1889, for the very simple reason that our available funds were not sufficient to provide the necessary expenses of another observer and instrument.

The eclipse negatives secured by Professor SCHAEBERLE in Chile showed a cometary form to which he called attention at the Astronomical Congress of the World's Fair in August, 1893.*

In May, 1894, we received, through the kindness of the Eclipse Committee of the Royal Astronomical Society, copies of the eclipse negatives taken by the British expeditions to Brazil and Africa, respectively. The eclipse-comet is found on both sets of plates (as well as on the plates of the HARVARD College Observatory expedition in Chile).

One of the incidental results of the expedition is, then, the discovery of a bright comet, which was seen at no observatory, and which would, therefore, have been lost had it not been for these plates. It should receive the designation of the "Eclipse-Comet of April 16, 1893," according to the scheme of nomenclature adopted by Professor KRUEGER in the *Astronomische Nachrichten*.

The most important conclusion reached by Professor SCHAEBERLE, at the eclipse of April, 1893, is that the photographs confirm his mechanical theory of the solar corona, announced in 1890. In his own words:

"The coronal structure of this eclipse is apparently governed throughout by a single simple law, viz: The matter is arranged along portions of elliptical streams, one focus of the ellipse being

* *Publications A. S. P.*, Vol. VI (1894), pages 144, 237.

at the Sun's center, * * * the origin of these streams being, in the main, confined to the Sun-spot regions."

The areas within the Sun-spot zones, projections from which produced the corona of April, 1893, have been located by Professor SCHAEBERLE. They are six in number; and outbursts of solar matter from them moving in ellipses will account for all the (main) features of the coronal prominences, wings and gaps as exhibited in the photographs.

"To sum up the results of the study relating more particularly to the corona of April 16, 1893, the following conclusions are arrived at:

"(1) All visible matter exterior to the Sun's surface is apparently in rapid motion, the path described corresponding to one of the conic sections. The so-called stationary prominences are probably stationary as to the general form only, the matter in the same being in rapid motion, which remains visible so long as the density is sufficiently great. I have carefully compared the eclipse photographs taken with the 40-foot telescope on April 16, 1893, with the best spectroscopic observations accessible (both visual and photographic) taken of the uneclipsed Sun on the same day, and the result, briefly stated, is that while the coarser features of the Sun's surroundings are shown in spectroscopic observations of the uneclipsed Sun, *the finer and essential details are wanting.*

"(2) All coronal streamers are apparently composed of matter ejected from the Sun with great velocity. This matter moves in one of the conic sections, and has its origin mainly in one of the Sun-spot zones. In general, when seen at the Moon's outline, in projection, any given portion of a coronal streamer is in reality at a great distance from the Sun.

"(3) A so-called wing of the corona is apparently produced by the overlapping in projection of many streams of matter coming from two (or more) different centers of eruption; the direction of the axis or greatest extent of the wing being in general roughly at right angles to the line joining the two eruptive centers. A necessary consequence resulting from the formation of wings is the formation of 'gaps' in the corona, a 'gap' being the less dense area between two adjacent wings.

"In conclusion, these investigations seem to demonstrate that, so far as the eclipse of April, 1893, is concerned, there is

no evidence that either electricity or magnetism has anything to do with the arrangement of coronal matter in the Sun's neighborhood. A re-examination of the photographs of other eclipses of which the observatory has copies leads to the same conclusion, viz : there is no necessity of introducing either electrical or magnetic forces to account for any of the coronal forms, as each true stream-line can practically be made to lie on the arc of a conic section having the sun at the focus. The abnormally curved streamers, usually forming wing-boundaries on the smaller scale photographs of the present eclipse are, on the larger negatives, shown to be due to the overlapping of different nearly osculating streamers, the individual members, however, can be made to coincide with the arc of a gravitational conic section. It is almost certain that the abnormal outlines recorded for other eclipses are produced in a similar manner."

LUNAR ECLIPSES AND SOME SIMILAR PHENOMENA.

BY W. H. S. MONCK.

The two most remarkable characteristics of lunar eclipses are the large amount of light which frequently appears on the eclipsed Moon and the fact that the eclipse—the true shadow—is larger than the geometrical shadow. The connection between these two facts has not perhaps been as fully recognized as it ought to be.

No one can doubt that the light which falls on the eclipsed part of the Moon comes from the Sun, and that it does not pass through the solid body of the Earth. Placing the corona out of account for the present therefore, the only explanation is that light, which would otherwise pass outside of the geometrical shadow, has been turned out of its direct course so as to fall within that shadow. This diversion of the light must naturally produce a deficiency at the place from which it has been diverted. And I think there can be little doubt that the cause which diverts the light is the Earth's atmosphere. Bending the rays which pass near the Earth into the geometrical shadow, it naturally leaves a deficiency of light for some distance outside of that shadow. The result is that the eclipsed Moon is at least partially illuminated, while the real shadow becomes larger than the geometrical shadow.

One consequence of this explanation is that great mountain-ranges like the Andes and Cordilleras, for example, will produce no effect on the terminator of the Earth's shadow on the Moon. The effect would be produced if we had no atmosphere, but the actual terminator does not correspond to the Earth's surface but to the atmosphere at a height of probably 50 or 60 miles, which is of course unaffected by the mountain-range. The mountains, by cutting off the light that would otherwise pass over the Earth nearly at the sea-level, will lessen the illumination of part of the eclipsed Moon, but this effect will take place not at the terminator but some distance inside of it. Whether the diminution would be sufficient to attract notice I cannot say. I can only point out where it should be looked for. Great banks of cloud would have the same effect—a darkening within the terminator. Bright and dark eclipses would, according to this view, depend mainly on the state of the atmosphere at the time. The distance of the Moon from the Earth and the breadth of the shadow at the place where the Moon crosses it would of course also affect the quantity of light bent into the centre of the shadow. There might be occasions when the bending was not sufficient to throw the light into the centre of the shadow if clouds or mountains intercepted the more strongly refracted rays. But the corona introduces a new element. The corona is known to vary from time to time in its dimensions, and I apprehend that the varying dimensions of the corona must affect the phenomena of a lunar eclipse.

Suppose, for simplicity, that the terrestrial atmosphere is removed, what is the condition of the part of the Moon immediately inside of the terminator? The Sun is cut off by the Earth, but the corona is still visible as we see it in a solar eclipse. The degree of illumination and the distance to which it extends inside of the geometrical shadow will depend on the brilliancy and extent of the corona, but the general effect will be that inside of the geometrical shadow there will be a ring of coronal light tracing out the shape and brightness of the corona on the eclipsed Moon. Now it is a remarkable fact that in many lunar eclipses there appears to have been a kind of ring within the terminator consisting of light differently colored from that which was visible nearer to the centre of the shadow. This ring, if examined with sufficient care, would I think have revealed the shape and brightness of the corona at the time of the eclipse. I hope, therefore, that the phenomena of lunar eclipses will be carefully studied in

future. The total phenomenon is a complex one, the effect of atmospheric refraction and coronal light being mixed together within the terminator; but with careful observation we might, perhaps, be able to discriminate the effects of each.

Let me apply the principles here considered to a transit of *Mercury* or *Venus*. If these planets have atmospheres, some of the light will be bent into the geometrical shadow, as in the case of the Moon. But, owing to their great distance from us, this light will, in most instances, not merely be bent into the geometrical shadow but will intersect it on the opposite side and get clear of it altogether. But the deficiency of light in the region from which it was bent into the shadow will remain, and if the planet or other object has an atmosphere, it will be surrounded by a ring less bright than the Sun or other body over whose surface it is passing. Similar results probably arise in the case of eclipse-stars, variables of the type of *Algol*. If the star which effects a transit has an atmosphere, there is a phenomenon similar to the enlargement of a lunar eclipse. It intercepts more light than its real magnitude will account for. Its atmosphere bends a number of the incident rays quite out of the visible direction of the star and they only reach us in the form of diffused sky-light. On the other hand, a star might be visible during a total eclipse if it had a very vivid corona, or, it may be added, if the eclipsing star was not quite dark.

The phenomena of the occultation of a fixed star by the Moon bear some similarity to those which I have been considering. The disappearance is usually instantaneous, proving, in the opinion of most astronomers, that the Moon has no atmosphere. Sometimes, however, the disappearance is slower. This may arise from the Moon's atmosphere not being of a permanent character. If our atmosphere consisted exclusively of aqueous vapor, a lunar astronomer would probably be much puzzled by the phenomena of terrestrial occultations. Sometimes a star seems to be visible after it has entered on the dark body of the Moon, as if its light was bent round the corner. Slow disappearance, however, may arise from the star being a close double or having a very large atmosphere (being possibly the nucleus of a small nebula) or a very brilliant and extensive corona. Considering the very small real discs of the fixed stars the Moon's angular motion is too rapid for making satisfactory observations. The occultation of a fixed star by a planet, when observed with a



powerful telescope, ought to afford more information both as to the planet's atmosphere and the nature of the star. Our own experience, however, shows us that we ought not to assume planetary atmospheres to be always clear. A lunar astronomer would often see a star occulted by the Earth, then reappearing and then occulted again—the first occultation being due to a bank of clouds which it cleared before getting behind the solid body of the Earth. Something like this is said to have occurred with *Jupiter* and may have arisen from this cause. Perhaps the best chance of obtaining information as to the real dimensions of the disc of a star would be an occultation by an asteroid, but such occultations are rare and difficult to predict. A star with a large corona or surrounded by a nebula might not totally disappear at any stage. Every observer has probably noticed that the discs of stars do not appear of the same size in the telescope, and that a fainter star often shows a larger disc than a brighter one. This may arise from the latter star being a close double or being surrounded by a nebula or a large and bright corona. If we could permanently shut out the body of the Sun from view, it would probably be taken as a kind of ring nebula.

In the case of some variable stars, I think there may be an occultation of a different kind. Their periods are very nearly equal to one year. This looks as if the obscuring body, whatever its nature may be, lay between the Earth and the star when we are at one part of our orbit, while at another part the intervening space is clear. The period, of course, might be a little lengthened or shortened by the mutual motions of the star and the obscuring body, the latter of which would probably be a nebula or meteor-swarm of considerable dimensions. This, however, is only conjecture, and I fear it will be difficult to obtain any information as to this class of occultations if they occur. A faint nebula apparently in the same direction as the star is probably the only indication that the most powerful telescope could detect. Such a nebula apparently surrounding a star which varied in a period of about a year would be a suggestive circumstance, but would hardly make out the case conclusively.

With regard to stars whose directions differ considerably from each other, it is still evident that the light of one is most probably, to a considerable extent, reflected or refracted when it falls upon another, giving rise to diffuse sky-light which cannot be traced to any particular star. While, therefore, the greater part of the

diffused light which we observe arises from atmospheric dispersion or from the direct light of stars which are too faint to be separately discernible, it is not wholly traceable to these causes. The light which is visible before a bright star enters the field of the telescope, however, is probably due almost entirely to the terrestrial atmosphere.

PREDICTIONS FOR THE TRANSIT OF MERCURY, NOVEMBER 10, 1894.

BY ROGER SPRAGUE.

TIMES OF INGRESS AND EGRESS (EXTERNAL CONTACT) FOR
UNIVERSITY PARK, COLORADO SPRINGS, PUEBLO,
AND TRINIDAD, COL.

UNIVERSITY PK.		COL. SPRINGS.		PUEBLO.		TRINIDAD.	
Lat. $39^{\circ} 40' 36''$		Lat. $38^{\circ} 50'$		Lat. $38^{\circ} 17'$		Lat. $37^{\circ} 11'$	
Long. $104^{\circ} 56' 54''$		Long. $104^{\circ} 49'.5$		Long. $104^{\circ} 36'$		Long. $104^{\circ} 30'$	
Log. ρ 9.999402		Log. ρ 9.999424		Log. ρ 9.999438		Log. ρ 9.999465	
INGRESS.	EGRESS.	INGRESS.	EGRESS.	INGRESS.	EGRESS.	INGRESS.	EGRESS.
A. M.	P. M.	A. M.	P. M.	A. M.	P. M.	A. M.	P. M.
M. S. T.	M. S. T.	M. S. T.	M. S. T.	M. S. T.	M. S. T.	M. S. T.	M. S. T.
H. M. S.	H. M. S.	H. M. S.	H. M. S.	H. M. S.	H. M. S.	H. M. S.	H. M. S.
8 55 59.2	2 12 16.94	8 55 58.91	2 12 16.46	8 55 58.96	2 12 16.07	8 55 59.18	2 12 15.45

UNIVERSITY PARK, Arapahoe Co., Colorado, Sept. 17, 1894.

PLANETARY PHENOMENA FOR JANUARY, FEB- RUARY AND MARCH, 1895.

BY PROFESSOR MALCOLM MCNEILL.

The following brief notes on the Sun, Moon and planets, have been prepared at the request of the Council, and are designed for the aid of those interested in astronomy and who do not have an almanac. In the descriptive paragraphs Pacific Standard time is given, unless the contrary is indicated.

JANUARY, 1895.

The Earth is in perihelion on January 2 at 4 P.M.

Mercury at the beginning of the month is a morning star too

near the Sun to be seen, passes inferior conjunction on the evening of January 9 and becomes an evening star. It moves rapidly away from the Sun and at the end of the month sets more than an hour later, so that it may possibly then be seen without telescopic aid under favorable conditions of weather.

Venus is also an evening star, setting about half an hour later than the Sun on January 1, and more than an hour later on January 31. It can probably be seen by a sharp eye soon after sunset after the first few days of the month; being very much brighter than *Mercury*, it can be seen much nearer the Sun. At the end of the month the two planets will be very close together. The nearest approach will be on the morning of February 1, when *Mercury* is only 35', a distance slightly greater than the Sun's apparent diameter, north of *Venus*.

Mars is getting farther away from opposition and by the end of the month sets at about 1 A.M. It is moving eastward among the stars in the constellation *Aries*, and during the month it moves about 13°. Its distance in millions of miles from the Earth increases from eighty-one to one hundred and eight. By the end of the month its light will be only about one-seventh as great as at opposition, but it will still be a conspicuous object in the sky.

Jupiter is in excellent position for observation, being above the horizon most of the night. It is in the constellation *Gemini* and during the month moves westward (retrogrades) about 3°.

Saturn is a morning star and by the end of the month rises just after midnight. It is in the eastern part of the constellation *Virgo* and moves eastward about 1° during the month. The ratio of apparent major to minor axis of the rings is about three to one.

Uranus is also a morning star rising somewhat after 1 A.M. on January 31. It is in the constellation *Libra* and moves eastward about 1°.

Neptune is in the eastern part of the constellation *Taurus* and retrogrades about 36'. It is above the horizon most of the night.

FEBRUARY.

Mercury is an evening star until February 25, when it comes to inferior conjunction and becomes a morning star. It reaches its greatest eastern elongation on February 9, when it sets about an hour and a half after the Sun. The first half of the month is about

the most favorable time of the year for seeing the planet. The next eastern elongation will occur early in June, and will also afford a good opportunity for seeing the planet, but perhaps not as good as at the present.

Venus is also an evening star, and is increasing its apparent distance from the Sun. At the end of the month it sets nearly two hours later. The relative motions of *Mercury* and *Venus* for the first part of the month are very interesting. Both planets are moving eastward among the stars at the beginning of the month. *Mercury's* motion is at first a little more rapid and it passes north of *Venus* 35' on the morning of February 1. It increases its lead until it is about 1° east of *Venus*, but its speed slackens, while *Venus* keeps up about the same rate and overtakes *Mercury* on the morning of February 10. On February 15 *Mercury* gives up the race, stops, and begins to move westward. At the time of the second conjunction, *Mercury* is 2° 38' north of *Venus*.

Mars sets earlier than it did during January. It moves about 17° eastward and northward from the constellation *Aries* into *Taurus*. Toward the close of the month it passes a few degrees south of the *Pleiades* group. Its distance from the Earth increases to 133,000,000 miles by the end of the month and it loses about one-third of its light during the month.

Jupiter sets before 3 A.M. at the close of the month. It is in the constellation *Gemini* and moves slowly westward until February 19 when it begins to move eastward, but the whole westward motion is only a little more than half a degree and the eastward motion not as much.

Saturn is gradually getting nearer a convenient time for observation. It rises by the close of the month at about 10 P.M. It is in the constellation *Virgo* and moves about 10' eastward until February 15, and during the rest of the month moves about 14' westward.

Uranus is in the constellation *Libra*, a little to the east of its January position. After February 22 it moves slightly westward.

Neptune remains in *Taurus* and retrogrades slowly.

MARCH.

The Sun crosses the equator and spring begins March 20, 1 P.M.

Eclipses: There will be two eclipses during the month.

A total eclipse of the Moon on March 10. The whole eclipse

will be visible throughout the United States, the beginning in the western portion of the eastern hemisphere and the ending in the Pacific Ocean. The middle of the eclipse comes at 7^h 39^m P.M. Pacific Standard time, and the total phase lasts 1^h 36^m.

A partial eclipse of the Sun on March 26 invisible in the United States. It may be seen along the west coast of Europe, in Iceland, Greenland and the northeastern portion of North America. The greatest magnitude of the eclipse is about one-third of the Sun's diameter. The time of greatest eclipse is 2 A.M. Pacific Standard time.

Mercury is a morning star throughout the month and comes to its greatest west elongation 27° 48' on the morning of March 24, but as it is far to the south of the Sun, the conditions for visibility are not good. About that time it rises about an hour before the Sun.

Venus is an evening star and is gradually increasing its distance from the Sun. At the end of the month it sets 2^h 22^m later than the Sun.

Mars is still an easily seen object in the western sky in the evening. On March 31 it sets about midnight. It is in the constellation *Taurus* and moves eastward about 18° during the month. Its distance from the Earth is rapidly increasing and at the end of the month it is about 161,000,000 miles. It will also diminish materially in brightness.

Jupiter will set before 1 A.M. at the end of the month. It is in the constellation *Gemini* and during the month moves about 2½° eastward.

Saturn by the end of the month will rise at a little after 8 P.M. It is in the eastern part of the constellation *Virgo*, and moves westward a little more than 1° during the month. The rings are open a little wider than they were in January; the ratio of minor axis of the ring to the polar diameter of the planet is about ¾.

Uranus rises a little more than an hour after *Saturn*. It is in the constellation *Libra* and moves slowly westward.

Neptune is in about the same position as before in the constellation *Taurus*. It is moving slowly eastward.

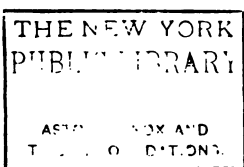
EXPLANATION OF THE TABLES. .

The phases of the Moon are given in Greenwich Mean time, which is 8 hours fast of Pacific Standard time. To find the corre-

sponding Pacific Standard time, subtract 8 hours from that given in the table. In the tables for Sun and planets, the second and third columns give the Right Ascension and Declination for Greenwich noon. The fifth column gives the local mean time for transit over the Greenwich meridian. To find the local mean time of transit for any other meridian, the time given in the table must be corrected by adding or subtracting the change per day, multiplied by the fraction whose numerator is the longitude from Greenwich, expressed in hours, and whose denominator is 24. This correction is seldom much more than 1^m. To find the standard time for the phenomenon, correct the local mean time by *adding* the difference between standard and local time if the place is west of the standard meridian, and *subtracting* if east. The same rules apply to the fourth and sixth columns, which give the local mean times of rising and setting for the meridian of Greenwich. They are roughly computed for Lat. 40°, with the noon Declination and time of meridian transit, and are intended as only a rough guide. They may be in error by a minute or two for the given latitude, and for latitudes differing much from 40° they may be several minutes out.

The tables of eclipses of *Jupiter's* satellites give in Pacific Standard time the eclipses which can be seen from some part of the United States. A few of those given occur before sunset in the extreme western part of the country, but after sunset in the east and may be seen there. The Roman numerals indicate the four old satellites in their order of distance from *Jupiter*. No agreement has yet been reached as to naming BARNARD's new fifth satellite, which is nearer the planet than is Satellite I, and no times of eclipse are given for it. As *Jupiter* is going from opposition to conjunction, the shadow of the planet lies to the east and all eclipse phenomena are to be seen on that side of the planet, those for Satellite IV being at a considerable distance from the disc. For I and II only reappearance (R) can be seen, the satellites entering the shadow while in occultation by the planet. For III and IV both disappearances (D) and reappearances may be seen, the disappearances for III being quite close to the planet's limb soon after its emergence from occultation. The eclipses of IV are the first since April, 1892. During the interval its distance from the planet and its inclination of orbit kept it clear of the shadow.





PHASES OF THE MOON, G. M. T.

		H. M.
First Quarter,	January 4,	7 52 A. M.
Full Moon,	January 11,	6 50 A. M.
Last Quarter,	January 17,	10 55 P. M.
New Moon,	January 25,	9 26 P. M.

THE SUN.

1895.	R. A. H. M.	Declination. ° ' "	Rises. H. M.	Transits. H. M.	Sets. H. M.
Jan. 1.	18 47	— 23 1	7 24 A.M.	12 4 P.M.	4 44 P.M.
11.	19 31	— 21 48	7 23	12 8	4 53
21.	20 14	— 19 54	7 20	12 12	5 4
31.	20 55	— 17 22	7 12	12 14	5 16

MERCURY.

Jan. 1.	18 25	— 24 43	7 12 A.M.	11 45 A.M.	4 18 P.M.
11.	19 36	— 23 38	7 36	12 13 P.M.	4 50
21.	20 47	— 20 1	7 53	12 45	5 37
31.	21 54	— 13 56	7 57	1 13	6 29

VENUS.

Jan. 1.	19 31	— 23 10	7 57 A.M.	12 37 P.M.	5 17 P.M.
11.	20 14	— 21 10	8 4	12 51	5 38
21.	21 6	— 18 7	8 4	1 4	6 4
31.	21 56	— 14 13	7 59	1 14	6 29

MARS.

Jan. 1.	1 52	+ 12 38	12 20 P.M.	7 7 P.M.	1 54 A.M.
11.	2 8	+ 14 13	11 52 A.M.	6 44	1 36
21.	2 25	+ 15 50	11 24	6 22	1 20
31.	2 45	+ 17 27	10 58	6 3	1 8

JUPITER.

Jan. 1.	6 0	+ 23 15	3 46 P.M.	11 14 P.M.	6 42 A.M.
11.	5 55	+ 23 16	3 2	10 30	5 58
21.	5 50	+ 23 17	2 18	9 46	5 14
31.	5 47	+ 23 18	1 35	9 3	4 31

SATURN.

1895.	R. A.		Declination.	Rises.		Transits.		Sets.	
	H.	M.		H.	M.	H.	M.	H.	M.
Jan. 1.	14	16	— 11 9	2 5	A.M.	7 30	A.M.	12 55	P.M.
11.	14	19	— 11 20	1 28		6 53		12 18	
21.	14	21	— 11 27	12 52		6 16		11 40	A.M.
31.	14	22	— 11 31	12 14		5 38		11 2	

URANUS.

Jan. 1.	15	5	— 17 4	3 14	A.M.	8 18	A.M.	1 22	P.M.
11.	15	7	— 17 11	2 38		7 41		12 44	
21.	15	8	— 17 16	2 0		7 3		12 6	
31.	15	9	— 17 20	1 22		6 25		11 28	A.M.

NEPTUNE.

Jan. 1.	4	50	+ 20 57	2 46	P.M.	10 5	P.M.	5 24	A.M.
11.	4	49	+ 20 56	2 5		9 24		4 43	
21.	4	48	+ 20 55	1 25		8 44		4 3	
31.	4	48	+ 20 54	12 45		8 4		3 23	

ECLIPSES OF JUPITER'S SATELLITES, PACIFIC TIME.

			H.	M.				H.	M.
I R.	Jan. 2,	12	9	A.M.	I R.	Jan. 17,	10	28	P.M.
I R.	3,	6	38	P.M.	II R.	18,	3	24	
II R.	3,	11	31		I R.	19,	4	57	
I R.	9,	2	4	A.M.	III D.	21,	5	3	
I R.	10,	8	33	P.M.	III R.	21,	7	49	
I R.	12,	3	1		II R.	22,	4	41	A.M.
III R.	14,	3	38		I R.	25,	12	23	
II R.	15,	2	6	A.M.	II R.	25,	5	59	P.M.
I R.	16,	3	59		I R.	26,	6	52	
IV D.	17,	5	35		III D.	28,	9	4	
IV R.	17,	6	8		III R.	28,	11	51	

PHASES OF THE MOON.

		H.	M.	
First Quarter,	February 3,	12	16	A. M.
Full Moon,	February 9,	5	23	P. M.
Last Quarter,	February 16,	1	9	P. M.
New Moon,	February 24,	4	44	P. M.

THE SUN.

1895.	R A. H. M.	Declination. ° ' "	Rises. H. M.	Transits. H. M.	Sets. H. M.
Feb. 1.	21 0	— 17 5	7 11 A.M.	12 14 P.M.	5 17 P.M.
11.	21 40	— 14 0	6 59	12 14	5 29
21.	22 18	— 10 32	6 47	12 14	5 41
Mar. 3.	22 56	— 6 48	6 32	12 12	5 52

MERCURY.

Feb. 1.	22 1	— 13 14	7 57 A.M.	1 15 P.M.	6 33 P.M.
11.	22 46	— 6 29	7 39	1 21	7 3
21.	22 41	— 4 31	6 48	12 36	6 24
Mar. 3.	22 6	— 8 30	5 40	11 15 A.M.	4 50

VENUS.

Feb. 1.	22 1	— 13 48	7 59 A.M.	1 15 P.M.	6 31 P.M.
11.	22 48	— 9 13	7 51	1 23	6 55
21.	23 34	— 4 13	7 40	1 30	7 20
Mar. 3.	0 19	+ 0 57	7 28	1 35	7 42

MARS.

Feb. 1.	2 47	+ 17 36	10 56 A.M.	6 1 P.M.	1 6 A.M.
11.	3 8	+ 19 9	10 31	5 42	12 53
21.	3 30	+ 20 34	10 8	5 25	12 42
Mar. 3.	3 54	+ 21 51	9 47	5 9	12 31

JUPITER.

Feb. 1.	5 46	+ 23 18	1 31 P.M.	8 59 P.M.	4 27 A.M.
11.	5 44	+ 23 18	12 50	8 18	3 46
21.	5 44	+ 23 20	12 10	7 38	3 6
Mar. 3.	5 45	+ 23 21	11 32 A.M.	7 0	2 28

SATURN.

Feb. 1.	14 22	— 11 32	12 10 A.M.	5 34 A.M.	10 58 A.M.
11.	14 23	— 11 32	11 31 P.M.	4 55	10 19
21.	14 23	— 11 29	10 52	4 16	9 40
Mar. 3.	14 22	— 11 23	10 10	3 35	9 0

URANUS.

Feb. 1.	15 9	— 17 21	1 18 A.M.	6 21 A.M.	11 24 A.M.
11.	15 10	— 17 23	12 39	5 42	10 45
21.	15 10	— 17 24	11 59	5 3	10 5
Mar. 3.	15 10	— 17 23	11 20 P.M.	4 23	9 26

NEPTUNE.

1895.	R. A. H. M.	Declination. ° ' "	Rises. H. M.	Transits. H. M.	Sets. H. M.
Feb. 1.	4 48	+ 20 54	12 41 P.M.	8 0 P.M.	3 19 A.M.
11.	4 47	+ 20 54	12 02	7 21	2 40
21.	4 47	+ 20 54	11 22 A.M.	6 41	2 0
Mar. 3.	4 47	+ 20 55	10 43	6 2	1 21

ECLIPSES OF *JUPITER'S* SATELLITES, PACIFIC TIME.

		H. M.			H. M.
I R.	Feb. 1,	2 19 A.M.	II R.	Feb. 16,	1 45 A.M.
II R.	1,	8 35 P.M.	I R.	17,	12 39
I R.	2,	8 47	I R.	18,	7 8 P.M.
IV D.	2,	11 25	II R.	19,	3 3
IV R.	3,	12 37 A.M.	IV D.	19,	5 23
I R.	4,	3 16 P.M.	IV R.	19,	6 59
III D.	5,	1 4 A.M.	I R.	24,	2 34 A.M.
III R.	5,	3 52	I R.	25,	9 3 P.M.
II R.	8,	11 10 P.M.	III R.	26,	3 56
I R.	9,	10 43	II R.	26,	5 38
I R.	11,	5 12	I R.	27,	3 32

PHASES OF THE MOON, G. M. T.

First Quarter,	March 4,	12 40 P. M.
Full Moon,	March 11,	3 38 A. M.
Last Quarter,	March 18,	5 32 A. M.
New Moon,	March 26,	10 25 A. M.

THE SUN.

1895.	R. A. H. M.	Declination. ° ' "	Rises. H. M.	Transits. H. M.	Sets. H. M.
Mar. 1.	22 49	— 7 34	6 36 A.M.	12 13 P.M.	5 50 P.M.
11.	23 25	— 3 42	6 20	12 10	6 0
21.	0 2	+ 0 15	6 3	12 7	6 11
31.	0 39	+ 4 10	5 44	12 4	6 24

MERCURY.

Mar. 1.	22 12	— 7 35	5 50 A.M.	11 28 A.M.	5 6 P.M.
11.	21 58	— 10 54	5 17	10 43	4 9
21.	22 21	— 11 12	5 1	10 26	3 51
31.	23 4	— 8 19	4 56	10 30	4 4

VENUS.

1895.	R. A. H. M.	Declination. ° '	Rises. H. M.	Transits. H. M.	Sets. H. M.
Mar. 1.	0 10	— 0 5	7 30 A.M.	1 34 P.M.	7 38 P.M.
11.	0 55	+ 5 5	7 19	1 40	8 1
21.	1 40	+ 10 5	7 7	1 45	8 23
31.	2 27	+ 14 42	6 58	1 52	8 46

MARS.

Mar. 1.	3 49	+ 21 37	9 51 A.M.	5 12 P.M.	12 33 A.M.
11.	4 13	+ 22 45	9 31	4 57	12 23
21.	4 38	+ 23 40	9 12	4 42	12 12
31.	5 3	+ 24 22	8 57	4 29	12 1

JUPITER.

Mar. 1.	5 44	+ 23 21	11 39 A.M.	7 7 P.M.	2 35 A.M.
11.	5 46	+ 23 23	11 2	6 30	1 58
21.	5 50	+ 23 25	10 26	5 54	1 22
31.	5 54	+ 23 27	9 51	5 19	12 47

SATURN.

Mar. 1.	14 22	— 11 25	10 19 P.M.	3 44 A.M.	9 9 A.M.
11.	14 21	— 11 16	9 38	3 3	8 28
21.	14 19	— 11 5	8 56	2 22	7 48
31.	14 17	— 10 52	8 14	1 40	7 6

URANUS.

Mar. 1.	15 10	— 17 24	11 28 P.M.	4 31 A.M.	9 34 A.M.
11.	15 10	— 17 22	10 48	3 51	8 54
21.	15 9	— 17 19	10 8	3 11	8 14
31.	15 8	— 17 14	9 28	2 31	7 34

NEPTUNE.

Mar. 1.	4 47	+ 20 55	10 51 A.M.	6 10 P.M.	1 29 A.M.
11.	4 47	+ 20 56	10 12	5 31	12 50
21.	4 48	+ 20 57	9 33	4 52	12 11
31.	4 49	+ 20 59	8 54	4 13	11 32 P.M.

ECLIPSES OF *JUPITER'S* SATELLITES, PACIFIC TIME.

		H.	M.			H.	M.
I R.	Mar. 4,	10	59 P.M.	I R.	Mar. 13,	7	24 P.M.
III D.	5,	5	5	I R.	18,	2	51 A.M.
III R.	5,	7	58	III D.	20,	1	6
I R.	6,	5	28	I R.	20,	9	20 P.M.
I R.	12,	12	55 A.M.	I R.	22,	3	49
III D.	12,	9	5 P.M.	I R.	27,	11	15
III R.	12,	10	49	I R.	29,	5	44
III R.	12,	11	59	II R.	30,	5	18

THE METEOR OF JULY 27, 1894, 7^h 30^m P. S. T.

BY EDWARD S. HOLDEN.

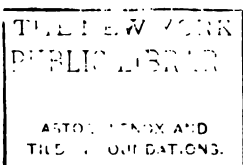
[Abstract.]

A great number of observations of this splendid meteor were received at the LICK Observatory, either directly, or through the kind offices of Hon. A. S. TOWNE, Hon. C. F. CROCKER, Hon. R. THOMPSON and others. Some of the best were not available till the middle of October. Very complete observations of the meteor's position and appearance were made at Mount Hamilton by seven observers. Professor AITKEN of the University of the Pacific sent the only *complete* observation received here, *viz.*, the altitude and azimuth of the meteor when it exploded, and the same co-ordinates of the point where it disappeared. Many good determinations of the point of explosion were received, the best being those of Professor FRIEND (Carson), Mr. CARLETON and Mr. BURCKHALTER (CHABOT Observatory), Mr. GEORGE BRAY (Santa Clara), Mr. W. B. JOHNSON and Mr. STONEROAD (Merced), Mr. HERROLD and Mr. HERRING (San José). Drawings of the brilliant cloud left by the explosion were received from Professor SCHAEBERLE (Mount Hamilton), Mr. JOHNSON (Merced), Mr. STEWART (Visalia); and excellent accounts of this cloud from Dr. O'BRIEN (Merced) and others. A beautiful and artistic water-color drawing of the phenomenon has been presented to the Observatory by Mr. CHAUNCEY M. ST. JOHN, which represents the general appearance in a most satisfactory way. The determinations of the place where the meteor disap-



THE METEOR OF JULY 27, 1894.

Drawn by CHAUNCEY M. ST. JOHN.



peared were not so satisfactory, the reasons being, no doubt, that everyone's attention was riveted on the cloud left by the explosion, and because the meteor divided into two portions near the end of its course.

When the last reports were received it was possible to fix with considerable precision the point where the meteor exploded. This point is in the zenith of a place about half a mile south and about half a mile west of the N. E. corner of T. 11 E., R. 8 S., M. D. B. and M. The explosion took place when the meteor was a trifle over 28 miles above the Earth's surface.

It is not so easy to fix the place where the meteor fell. Bakersfield, Hollister, Los Gatos, Madera, Merced, Minturn (two observers), Mount Hamilton (E. S. H.), San José (two observers), report that the meteor moved *north* as it fell.

College Park and Mount Hamilton (R. H. T. and A. F. P.) report that it moved south in falling.

Borden, Carson, Crow's Landing, Fresno, Livermore, Los Baños, Oakland (CHABOT Observatory), Riverside, San Andreas, Santa Clara, Vallejo, Visalia, report the direction of its fall as substantially vertical; and, in default of more accurate data, I have assumed this to represent the facts.

The meteor *disappeared* before it reached the Earth. The fragments produced by the explosion were probably volatilized by the time they reached a height of some 6 miles above the Earth's surface. It is hardly likely that any large fragments reached the Earth. If so, they should be sought for within a circle of some 12 miles in diameter, whose centre is the point previously described. If the observations of the point where the meteor disappeared had been more accurate, the place of the fall would have been better determined.

The meteor was seen by several observers long before it exploded—probably when its height was over 70 miles. The position of the radiant-point is R. A. $16^{\text{h}} 0^{\text{m}}$, Declination $+34^{\circ} 5'$.

With these data I have calculated the orbit of the meteor. As the data are (in general) not very precise the elements of the orbit are but approximate. The orbit is assumed to be a parabola. The elements are :

$$\begin{aligned}\pi &= 130^{\circ}, \\ \Omega &= 125^{\circ}, \\ i &= 20^{\circ}, \\ q &= 1.016.\end{aligned}$$

The meteor was then a little nearer the Sun than the Earth;

it was near perihelion, and moving in an orbit inclined some 20° to the ecliptic.

The Regents of the University have authorized the printing of a pamphlet on this subject, which will (in due time) be sent to our correspondents. This pamphlet will also contain the observations of the August meteors of 1894 made by Professor SCHAEBERLE at Monte Diablo, and by Messrs. COLTON, PERRINE and POOLE, at Mount Hamilton.

LICK OBSERVATORY, October 20, 1894.

THE PROBABLE REVOLUTION OF THE ORBIT OF THE INNER SATELLITE OF *MARS*.

BY W. W. CAMPBELL.

My observations of the position of *Phobos* with reference to the planet show that the eastern elongations occur at considerably greater distances from the planet than the western elongations do. Professor HALL'S observations in 1877 showed unmistakably that the western elongation distances were then considerably greater than the eastern. The relative positions of the Earth and *Mars* at the times of the 1877 and 1894 measures were slightly different, but that fact accounts for only a very small part of the change. The greater part of the change has probably been produced by a revolution of the position of the major axis of the orbit of *Phobos*.

There are strong indications from the observations of *Deimos*, the outer satellite, that the orbit of that satellite has been similarly transformed; but the evidence is not so conclusive as in the case of *Phobos*, since the orbit of *Deimos* is nearly circular.

LICK OBSERVATORY, November 15, 1894.

OBSERVATIONS OF THE TRANSIT OF *MERCURY*,
NOVEMBER 10, 1894, AT THE CHABOT
OBSERVATORY.

BY CHARLES BURCKHALTER.

Contacts III and IV of the transit of Mercury were observed at this observatory with the 8-inch equatorial, with power of 200.

Contacts I and II were lost on account of a dense fog, the Sun not being visible until 9:30 A.M.

The following gives the times expressed in Pacific Standard time, 8 hours slow of Greenwich :

Contact III. $1^h 11^m 39^s.0$

IV. $1^h 13^m 18.5$

The seeing at contact III was fair, and I consider the observation reliable. The seeing at contact IV was very poor, and a $\pm$ error of 3 to 5 seconds must be admitted.

OAKLAND, November 14, 1894.

OBSERVATIONS OF THE TRANSIT OF *MERCURY*,
NOVEMBER 10, 1894, AT ST. HELENA, NAPA
COUNTY, CALIFORNIA.

BY ALLEN F. GILLIHAN.

[Abstract.]

Contact I. $19^h 57^m 27^s$ P. S. T. Seeing very poor, the Sun is low, though the sky is clear.

Contact II. $19^h 58^m 42^s$ P. S. T. Both contacts I and II are not well determined.

Contact III. $1^h 11^m 30^s$ P. S. T. Seeing much better; *Mercury* is intensely black. No circle of light noticed; the black drop was seen.

Contact IV. $1^h 13^m 14^s$ P. S. T.

The observations above were made with a 3-inch equatorial refractor, as follows : I and II were observed by projection,

making the Sun's disc 8 inches in diameter; III and IV were observed with a HERSCHEL sun-prism and eyepiece magnifying 150 diameters. The clock corrections are from observations by myself with a portable transit instrument of $1\frac{3}{4}$ inches aperture.

OBSERVATIONS OF THE TRANSIT OF *MERCURY*,
NOVEMBER 10, 1894. AT COLLEGE PARK,
UNIVERSITY OF THE PACIFIC.

BY PROFESSOR R. G. AITKEN.

[Abstract.]

Contact I. Lost in the fog.

Contact II. $19^h 58^m 48^s.6$ P. S. T. This time is certainly too late; possibly as much as 10 or 15 seconds. When the planet was first seen, free of the fog, it had already entered on the disc and the ring of sunlight around it was complete.

Contact III. $1^h 11^m 34^s.3$ P. S. T.

Contact IV. $1^h 13^m 18^s.0$ P. S. T.

The observations were made with the 6-inch equatorial of the college observatory. The clock corrections are from observations by myself with the portable transit instrument.

OBSERVATIONS OF THE TRANSIT OF *MERCURY*,
NOVEMBER 10, 1894, IN SAN FRANCISCO.

BY F. R. ZIEL.

Professor E. S. HOLDEN,

Director LICK Observatory, Mount Hamilton.

Dear Sir: The transit of *Mercury*, November 10, was observed by me with my $2\frac{7}{8}$ -inch refractor, with a power of 150,

by projection on a cardboard screen, on a scale of 12 inches for the diameter of the image of the Sun.

Owing to a dense fog contacts I and II could not be observed, the Sun remaining obscured until 10 o'clock.

The observations of contacts III and IV were as follows :

III. $1^h 11^m 23^s$ P. S. T.

IV. 1 13 2

The seeing was fairly good. The time was noted by Mr. F. H. McCONNELL, who received the correct time at noon, direct from the LICK Observatory. Very truly yours,

F. R. ZIEL.

SAN FRANCISCO, November 10, 1894.

CONCERNING AN ATMOSPHERE ON *MARS*.*

—
BY W. W. CAMPBELL.
—

In forming an estimate of the extent of the atmosphere surrounding the planet *Mars*, a great many classes of observed phenomena must be considered. I shall try, in the following pages, to bring together and to discuss very briefly the most important facts bearing upon this question.

(1) *The Small Mass of the Planet.*—It is reasonable to suppose that the mass of the atmosphere surrounding a planet is roughly proportional to the mass of the planet itself. The largest planets would have the most extensive atmospheres. This rule actually appears to hold good. There is no evidence of an atmosphere on our Moon, nor on any of the satellites in the solar system. There is substantially no evidence of an atmosphere on the small planet *Mercury*, but there are unmistakable evidences of extensive atmospheres on the large planets *Jupiter*, *Saturn*, *Uranus* and *Neptune*, compared with which our own atmosphere is very slight.

Now, the mass of *Mars* is only 0.11 that of the Earth. The area of the surface is 0.28 that of the Earth. If the atmospheres of the two planets are proportional to their masses the quantity

* This question relates simply to the *extent* and *character* of the Martian atmosphere. Whether or not an atmosphere of any given extent and character will support life is quite outside of the astronomer's province.

of air above a square mile on *Mars** would be only 0.39 of the atmosphere above a square mile on the Earth.

(2) *The Color of the Planet*.—Many observers consider that the reddish-orange color of *Mars* is caused by the action of its atmosphere, in absorbing the rays at the violet end of the spectrum more strongly than it does those at the red end. It has even been said that the Earth would probably exhibit the same tint if it could be seen from without. The red color of the Sun near sunset, and the copper color of the Moon when near the horizon, are well-known instances of the strong absorption of the blue and violet rays by the thick stratum of atmosphere through which their light must travel. From recent comparisons of the colors of *Mars* as seen in the telescope and of the Moon as seen near the horizon by naked eye, I estimated that the colors of the two bodies approached most nearly to equality when the planet was on the meridian at altitude 60° and when the Moon was from 3° to 5° above the horizon.† At those altitudes the lunar light traversed paths in our atmosphere from nine to sixteen times as long as that traversed by the light from *Mars*. That is, if the orange hue of *Mars* is caused by an atmosphere similar to our own, it must be many times as extensive as our own. Some of the sunlight reflected to us by the planet has passed twice through its atmosphere, but much of it would pass only partially through before being reflected. It would seem, therefore, that this explanation of the red color of the planet would require an atmosphere from six to twelve times as extensive‡ as our own.

We shall see, later on, that all the other observed phenomena are opposed to that theory. In this section I shall mention only three :

First.—The edge of the disc is always *whiter* than the centre. This is an easy observation to make, and the fact has been recorded by many observers. Among others, it was noticed by

* The density of the atmosphere at the surface of *Mars* is also of interest. The force of gravity at the surface of *Mars* is only 0.38 of the force of gravity at the Earth's surface, and the density of the atmosphere at *Mars'* surface would be only 0.15 as great as at the Earth's surface. That is, less than one-half the density of our atmosphere at the summit of the Himalaya Mountains.

† This estimate is of course only roughly approximate. The observation should be made by a reflector, which is free from chromatic aberration. The light from *Mars*, also, appears to have a distinctive tint which cannot be exactly matched by lunar light.

‡ By this I mean that above any given area on *Mars* (a square mile, for instance), there would have to be from six to twelve times as many molecules of atmosphere as there are above an equal area on the Earth.

DAWES, who wrote in 1864 : " Nothing, as it appears to me, can more fully prove that the ruddy tint of *Mars* does not arise from any peculiarity in the color of the planet's atmosphere, than the fact that the redness is always deepest near the centre, where the atmospheric stratum is thinnest." In other words, if the redness at the centre is produced by the atmosphere, the greater thickness of the atmosphere at the edge should cause *increased* redness. Observation shows that exactly the contrary is true.

Second.—There is no evidence of absorption by the atmosphere over the polar caps. I think all observers have assigned to them a pure snow-white color, even at the edge of the planet. If the red color at the centre of the planet were due to atmospheric absorption, the absorption at the edge of the disc would be enormously greater, and the polar caps would not be pure white. They would incline to the yellow or red, very much as the rest of the planet does.

Third.—The spectra of the large planets *Jupiter*, *Saturn*, *Uranus* and *Neptune* contain prominent atmospheric bands, showing unmistakably that those planets are surrounded by extensive atmospheres. In *Mars* the spectroscopic evidence, as we shall see in the following section, is, to say the least, extremely slight. Yet *Mars* is vastly redder than the four outer planets. The spectroscopic evidence is therefore undeniably against the theory that the red color of *Mars* is caused by an extensive atmosphere.

In view of the facts that the limb of the planet is *whiter* than the central regions, that the white polar caps present no evidence of absorption even at the very edge of the disc, and that the larger planets with more extensive atmospheres than *Mars* are whiter than *Mars*—in view of those facts we must accept as satisfactory the explanation offered by Sir JOHN HERSCHEL half a century ago, that the red color " indicates, no doubt, an ochrey tinge in the general soil, like what the red sandstone districts on the Earth may possibly offer to the inhabitants of *Mars*, only more decided."

(3) *Spectroscopic Results.*—An account of the spectroscopic observations of *Mars* is contained in No. 37 of these *Publications* and the reader is referred to that paper for details. In brief, the investigations of JANSSEN about 1867, HUGGINS in 1867, SECCHI about 1872, VOGEL in 1872, and MAUNDER in 1877, led them to

substantially the same result, viz: *The atmosphere of Mars is similar to our own.* It must be noticed that none of the observers, so far as I know, formed from their spectroscopic evidence any estimate of the *extent* of the Martian atmosphere as compared with our own. One of the observers stated that the critical lines and bands were stronger in the planet's spectrum than in the lunar spectrum when these objects were at the same altitude above the horizon; another stated that the critical bands were broader in *Mars'* spectrum than in the Moon's, and another observer did not see the critical lines in the lunar spectrum at all, but did see them in the planet's spectrum. Now, if the atmosphere of *Mars* is similar to our own, and if at the same altitude the critical lines are stronger in the planet's spectrum than in the Moon's, there ought to be some *lower* altitude of the Moon such that the critical lines would be of equal strength. By this method—and by no other spectroscopic method, so far as I know—an estimate of the extent of the Martian atmosphere can be formed. It likewise would afford a test of the delicacy of the spectroscopic method. None of the observers seem to have sought for those (unequal) altitudes of *Mars* and the Moon for which the critical lines in the two spectra would be of equal intensity. It is unfortunate that this was not done.

It should also be noted that the observers found no certain evidence of increased absorption at the edge of *Mars*, where the thickness of atmosphere would be greatest.

The spectroscopic observations made at Mount Hamilton this summer, under especially favorable circumstances, showed that the spectra of the Moon and *Mars* were apparently identical, and that the method was sufficiently sensitive to have shown the existence of a possible Martian atmosphere about one-fourth as extensive as our own.

Thus the early observations of the spectrum of *Mars* gave some evidence of an atmosphere, but none of the observers have given us an estimate of its extent. The recent observations gave no evidence of an atmosphere, and were apparently able to detect an atmosphere if it were one-fourth as extensive as our own.

(4) *Distinctness of Surface Markings.*—If *Mars* were surrounded by an atmosphere as extensive as our own it is hardly possible we should see the surface features as clearly as we do. LANGLEY found that about 40 per cent. of the light coming to the Earth's surface from a celestial object near the zenith is

absorbed by our atmosphere. If the reflecting power of the Earth's surface is equal to that of the Moon, which is about 0.17, then of the 60 per cent. of the original light reaching the Earth's crust, only 0.17, or ten parts of the original hundred, would be reflected, and about 40 per cent. of this would be absorbed in passing out through our atmosphere. That is, only about 6 per cent. of the light originally striking the Earth's atmosphere would both reach and pass out directly from the Earth's crust. The surface would, however, be illuminated by the diffused sky light, and the light passing out from the surface to surrounding space could perhaps be estimated at 9 or 10 per cent. The light by which a distant observer, say on *Mars*, would view the Earth, would come very largely from the bright atmosphere and only very slightly from the real surface of the Earth. It is doubtful whether any of the Earth's surface features would be seen at all, even were our sky clear from clouds.

It has been urged that the *indistinctness* of the surface markings at the edge of *Mars* is due to the fact that we are there looking through great depths of atmosphere. I do not think this assumption is necessary. If we examine the lunar surface by naked eye we shall find that the markings are distinct at the centre, but become indistinct at the border. The Moon by naked eye is comparable in size with *Mars* as seen in a telescope. If we make allowance for the highly magnified effect of poor seeing in examining the blurred and indistinct edge of *Mars*, we shall find, I believe, that the edge of *Mars* is not greatly more indistinct than is the edge of the Moon. That the markings cannot be followed even up to the edge of the Moon and *Mars* is due mostly to foreshortening.

(5) *The Polar Caps*.—The excessive brightness of the polar caps, as compared with the central areas, at once renders the theory of a thick atmosphere untenable. If there *were* a thick atmosphere it would be extremely thick at the edge, where the polar caps are. Comparatively few of the Sun's rays would penetrate far enough to strike the material composing the caps. Those which did strike the caps would be largely reflected; but very few of these rays, in turn, would succeed in getting back through the thick atmosphere. Those portions of the polar caps nearest the edge of the disc would not have their present excessive brightness. This brightness can be explained only on the supposition that the planet's atmosphere is very thin.

The waxing and waning of the polar caps with the winters and summers on *Mars*, combined with their white color, have led most observers to suppose that they are composed of snow and ice. This view suggests at once that *Mars* has an atmosphere containing aqueous vapor. The caps are certainly analogous to the Earth's polar caps, so far as their waxing and waning and white color are concerned; but the analogy really extends no further.

An observer on the Moon or on *Mars*, looking at the Earth, would be hindered in his observations not only by our atmosphere, but by our clouds. The hemisphere turned toward him would never be wholly clear. We know that at times practically the whole of the central and eastern portions of North America are covered with clouds which at the same time extend far out to sea. To an outsider the cloud areas would be brighter than the clear areas. The contrast between clouded and clear regions would be greater than between land and water. We are by no means certain that our permanent boundary lines of land and water would be visible to an outsider, even in clear weather, on account of the much brighter sky covering them; but let us suppose them to be. They certainly would be complicated and lost in our cloudy weather. It is probable that our polar regions are enveloped in clouds more than half the time; in the temperate zone the proportion is less than one-half; while in certain equatorial regions there is almost perpetual cloudiness.

The conditions on *Mars* are very different. We are not justified in holding that clouds have ever been seen on that planet. The polar caps wax and wane. If the caps are composed of snow, when they are melting we would expect to see clouds in the polar regions. We have absolutely no evidence that clouds ever appear there. On the contrary, there is conclusive evidence that the margins of the polar cap are clear of clouds for at least weeks and months. I refer to the evidence of the bright projections jutting out from the edge of the polar cap upon the adjacent darker regions of the disc. These projections and the similar detached portions of the cap, remain constantly visible for weeks with substantially no change of form, and no change in definiteness of outline. Similarly there is no conclusive, or even strong, evidence that clouds ever form in the equatorial regions of *Mars*. To a distant observer the evidence of clouds on the Earth must be unmistakable. So

far as we know, the Martian inhabitant is not aware that a cloud can exist! If the conditions of our own atmosphere are good, the principal contour lines on *Mars* are always visible. When we begin observations for the night we do not ask: Is *Mars* clear to-night? but always: Is our own atmosphere tranquil? We do not stop work because *Mars* has clouded over, but because our own atmosphere is cloudy. *Mars* appears to be always clear.

The conclusion to be drawn from this is, that since there is no evidence of clouds there is no evidence from the polar caps that aqueous vapor* exists in an atmosphere, or that any circulation of the materials forming the polar caps takes place in an atmosphere.

There is another important piece of evidence which must not be overlooked. The planet *Mars* is much further from the Sun than we are. The intensity of the Sun's heat and light are only three-sevenths as great on *Mars* as on the Earth; and yet its climate *seems* to be milder than our own. Not only do the polar caps in the winters not extend so near the equator as they do on the Earth, but they sometimes disappear entirely under the influence of the summer sun. The south polar cap disappeared entirely about the middle of last October. It would be an easy matter, apparently, for an arctic explorer to reach the poles of *Mars*. While this condition of things can possibly be explained by the fact that their summers are very long, we must not forget that their winters are also very long, and the accumulation of snow during their winters should be correspondingly great. If the polar caps are snow, we must consider that the Martian climate is milder than our own. But, on that basis, how can we explain the absence of clouds?

Several observers have pointed out that the polar caps may consist of frozen crystals of carbonic acid. This theory† has

* In concluding my paper on "The Spectrum of *Mars*," *Publications A. S. P.*, page 236, I said that "While I believe that the polar caps on *Mars* are conclusive evidence of an atmosphere and aqueous vapor, I do not consider that they exist in sufficient quantity to be detected by the spectroscope." I now think that the expression "conclusive evidence of an atmosphere and aqueous vapor" is too strong. In making the spectroscopic observations I was constantly holding in mind the idea that the polar caps *were* snow, that the spectrum *ought* to show the aqueous vapor lines, and in the beginning I had expected to find it an easy matter to *confirm* the old observations. It is difficult to give up ideas once firmly held.

† Professor HOLDEN and other astronomers have often remarked that while analogy suggests the theory that the polar caps are snow, there are other substances whose properties are such that they may possibly satisfy the existing conditions better than snow does. One of our members, Mr. HENRY H. BATES, has written an interesting and suggestive letter on the carbonic acid theory of the caps. His letter is published on another page of these *Publications*.

much in its favor. Carbonic acid may be both liquefied and frozen when subjected to intense cold. It may be frozen into a transparent ice-like mass, or into white snow-like flakes. The white flakes would retain their form and color so long as the temperature was lower than 109° below zero Fahrenheit! When the temperature would rise above -109° the white flakes would be transformed into a colorless vapor. If the caps were composed of the white flakes of carbonic acid, they would wax and wane under the influence of the Sun's heat just as they are observed to do. We would be relieved from the necessity of considering that *Mars* has a milder climate than the Earth has. The absence of clouds would be explained. The absence of spectroscopic evidence of atmosphere and aqueous vapor would be explained. The clearness of view and the white color of the polar caps would be explained. Carbonic acid gas is a prominent constituent of our atmosphere, and it is not unreasonable to suppose that it exists abundantly also on *Mars*. It may even be the principal constituent of *Mars'* atmosphere. The late Dr. STONEY has elaborated an ingenious theory to account for the absence of atmosphere on the Moon, and in fact to explain how any small planet may gradually lose its atmosphere. His theory is based upon the accepted dynamical theory of gases. According to the dynamical theory of gases, all the atoms or molecules making up a gas are in constant and violent motion. The little particles fly with tremendous speed, in some cases exceeding the velocity of rifle balls. If there were no resisting force the particles would fly on and on, and the gas would be almost instantly dissipated and lost. In the case of a planet's atmosphere, the force of gravity is the force which overcomes the velocities of the particles and keeps them from flying off into space. The large planets attract their atmospheres more strongly than the small planets do, and the heavy gases are more strongly attracted than are the lighter gases. Dr. STONEY considered that the maximum velocities of the atoms of hydrogen, nitrogen and oxygen are so great, that if these gases ever existed on the Moon the gravity of the Moon would be too small to overcome those velocities. The particles would, one by one, fly off into outer space, never to return. He considered the fact that the Moon has no appreciable atmosphere could be accounted for in that way. Now, hydrogen is the lightest of the gases and its atoms fly about with greater velocity than the atoms of any of the other gases. Dr.

STONEV considered that if free hydrogen existed in our atmosphere the attraction of the Earth upon it would not be sufficient to hold it. The free hydrogen would, atom by atom, fly away into outer space. He considered that our oxygen and nitrogen were too heavy to escape. Now, *Mars* is intermediate in mass between the Earth and Moon. If all of the constituents of the Moon's atmosphere could escape its attraction, and if free hydrogen could escape the Earth's attraction, it is probable that all but the heavier gases on *Mars* could escape the attraction of that planet. Now, in our atmosphere it happens that water vapor is the lightest; the combined nitrogen and oxygen come next in weight; and the carbonic acid gas is the heaviest of all. There might come a time on *Mars*—the present time, for instance—when the carbonic acid constituted the greater part of its atmosphere. If such were the case, the temperature of the surface of the planet would probably be very low, low enough to freeze the carbonic acid into snow-like flakes. [It is for physicists, rather than for astronomers, to decide upon the merits of Dr. STONEV's theory. See Sir ROBERT BALL's interesting account of this theory in *Publications A. S. P.*, Vol. V, pages 25-29.]

But it is not necessary that we send the atmosphere of *Mars* out into space. It might easily go in exactly the opposite direction. That planet is much smaller than the Earth. It is probably older than the Earth, but we do not know it is. At any rate, being smaller, it would cool off in a small fraction of the time required by the Earth. It has probably gone far beyond the present condition of the Earth. As a planet grows older it is but natural that more and more of the oxygen in its atmosphere should be taken up by the constituents of its crust. It is pretty certain that more and more of our oxygen is taken up by the iron and other elements in the Earth's crust to form the oxides of those elements. It is possible that other elements of our atmosphere are gradually passing into the Earth's crust. Possibly the carbonic acid is left behind and in that way is becoming relatively more plentiful. Possibly processes similar to these have been going on in the atmosphere of *Mars*. Thus, if *Mars* originally had an extensive atmosphere, only a small residue of it might now remain, and its character might have become radically changed. It is possible, too, that other elements of the Martian atmosphere may assume the snow-like form and color under the influence of extreme cold. The view that the polar

caps consist of snow, and are therefore evidence of aqueous vapor in the atmosphere, leads us into great difficulties. Moreover, that view of their composition is not *necessary*. The caps may consist of carbonic acid, or of some other substance very different from our snow. [Considerable space has been given to the carbonic acid theory in this paper, not because the writer accepts that theory, but only for the purpose of calling attention to the fact that we are not limited to the one theory that the caps consist of snow and ice.]

It is not unreasonable to suppose that the characters of the Martian and terrestrial atmospheres may be very different. The planets are passing through processes of evolution covering millions of years. *Mars* and the Earth differ in mass and distance from the Sun. The probability that at the present time the conditions on *Mars* are approximately the same as those on the Earth is very slight indeed.

(6). In many respects *Mars* resembles the Moon; which has no appreciable atmosphere. First, the reflecting power of the surface of *Mars* is very small. It is but little larger than that of our Moon, or than that of *Mercury*, which presents no certain or strong evidence of an atmosphere. It is very much less than for the four large planets on which we know there are very heavy atmospheres. Second, the edge of *Mars* is much brighter than the inner portions of the disc, just as on the Moon, and probably for the same reasons, viz: The mountain surfaces on the two bodies reflect light more strongly than do the plains. The reflecting surface at the centre of the disc in each case is composed of both mountains and plains; while at the edge of the disc the reflecting surface is composed almost wholly of mountains, since the valleys have passed out of sight. That the increased brightness is due to surface features and not to atmosphere is shown by the fact that the brightening at the edge of *Mars* is not uniform, and neither is it on the Moon. Then, too, on those planets which we know are surrounded by an extensive atmosphere, *Jupiter* and *Saturn* for instance, the edges are actually much *fainter* than the centres of the disc are.

ZÖLLNER has estimated that if the greater brightness at the edge of *Mars* is due to mountains, they must be exceedingly steep—that their slopes must make an angle of 76° with the horizon. I have no hesitation in expressing the opinion that ZÖLLNER's estimate is excessive. I do not think the edge of *Mars* is

relatively brighter than the edge of the Moon, and do not think that excessively steep slopes are required. Some of the apparent brightness of *Mars* is unreal, and due to contrast with the dark sky background,* as Professor ORMOND STONE and others have pointed out; and some of it is real, and probably due to the greater reflecting power of the mountainous areas making up the visible edge of *Mars*.

(7). At the last three oppositions of *Mars*, we have observed bright projections jutting out from the terminator of the planet. These projections were probably high mountain peaks.† If they were mountains, they could not possibly have been seen if they had been placed at the bottom of a thick atmosphere. Either the projections are not mountains or the atmosphere is thin.

There are light and dark areas on *Mars*. If we make proper allowance for the high magnifying power used on *Mars*, the contrasts between the light and dark shades on that planet seem to be about the same as the contrasts between the light and dark regions on the Moon. The light and dark regions on the Moon are both land. Most astronomers hold that the bright markings on *Mars* are land and that the dark markings are water; but several astronomers hold exactly the opposite view. Since there is very slight, if any, evidence of aqueous vapor or clouds on *Mars*, it seems to me that there can be no fatal objection to the working hypothesis that both bright and dark markings may for the *most* part be land.

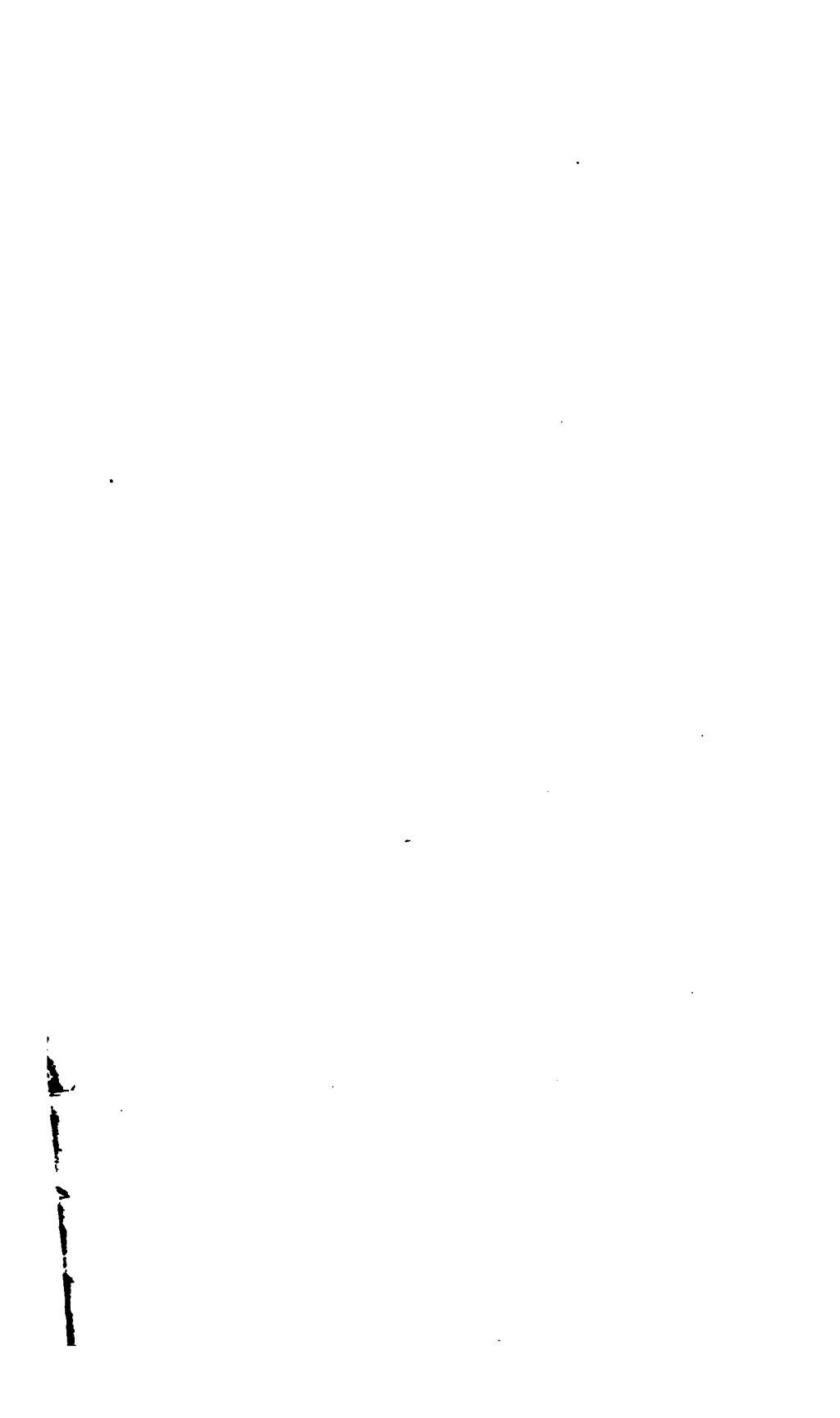
It may be true that changes are going on from time to time in the outlines of the so-called continents and so-called canals. One must systematically observe the planet at a great many oppositions if he wishes to give an opinion of any value on that subject. However, the longer I observe the planet, the stronger becomes the opinion that the varying conditions of our own atmosphere and the different distances and positions of the planet are responsible for most, if not all, of the differences between the drawings made by an observer at different times.

In conclusion, it seems to me that so far as the atmosphere of the planet is concerned, the conditions approach more nearly those existing upon the Moon than those existing upon the Earth.

MOUNT HAMILTON, November, 1894.

* This is shown conclusively to be the case by making the observations after sunrise, when the sky background is bright.

† For the history of this subject, see *Publications A. S. P.* for 1894, pages 103-112.





1894, June 30, 15^h 30^m.
 $\lambda = 356^{\circ}3$; $\phi = -22^{\circ}9$.



1894, September 4, 14^h 5^m.
 $\lambda = 65^{\circ}0$; $\phi = -16^{\circ}4$.



1894, August 22, 14^h 30^m.
 $\lambda = 192^{\circ}4$; $\phi = -17^{\circ}3$.



(X is a defect in the paper.)
 1894, September 19, 13^h 43^m.
 $\lambda = 282^{\circ}4$; $\phi = -16^{\circ}4$.



1894, August 30, 14^h 30^m.
 $\lambda = 117^{\circ}5$; $\phi = -16^{\circ}7$.



1894, October 3, 10^h 10^m.
 $\lambda = 165^{\circ}3$; $\phi = -17^{\circ}5$.



NOTICES FROM THE LICK OBSERVATORY.

PREPARED BY MEMBERS OF THE STAFF.

BRIGHT PROJECTIONS AT THE TERMINATOR OF *MARS*.

The writer of an unsigned article in *Nature* for August 2, 1894 (page 319) was apparently first made aware of the fact that bright prominences are to be seen at the terminator of *Mars* by a note printed in the *Astronomische Nachrichten*, No. 3245, describing such observations at the Observatory of Nice on July 28. If he had consulted *Astronomische Nachrichten*, No. 3241, a month earlier, he might have read an account of their observation at the LICK Observatory on June 28. The *Publications* of the Astronomical Society of the Pacific, Vol. VI, page 103, contains a paper by Professor CAMPBELL which recites the whole history of such appearances, which should be, one would think, fairly familiar to observers of *Mars* by this time. True prominences were first observed at the LICK Observatory in 1890; and at the LICK Observatory, at Nice, and at the Arequipa Observatory in 1892. The first prominences of 1894 were seen at Mount Hamilton on June 28, and they have been observed more or less constantly since that time, and will probably be seen after opposition also.

Bright prominences on *Mars* are of two kinds. The first kind is caused by pure irradiation of a bright region on the terminator over into the adjacent darkness. Such prominences are, therefore, entirely optical in their origin. M. TERBY, in 1888, described such appearances; and they had long been known in connection with the bright polar caps. The second kind of prominences corresponds to elevated regions, highly illuminated, on the planet's surface, which actually project beyond the terminator. They may, conceivably, be due either to clouds or to mountains. The Mount Hamilton observations of 1890 might very well have been explained by the presence of a streak of

clouds at a great altitude. (See *Publications* A. S. P., Vol. II, page 248.)

All the later observations made here by Professors SCHAEBERLE, CAMPBELL, and others, seem to indicate that these veritable prominences are caused by mountain chains lying across the terminator. The prominences appear in the same longitudes and latitudes on the planet night after night and even month after month; and a map of some of these chains is in preparation.

Professor CAMPBELL has shown that the heights of some of these mountains are not over 10,000 feet, and the heights of others appear to be of the same order of magnitude. At the LICK Observatory and at Nice, no observations of bright prominences at the full limb of *Mars* have been made.

There is not the slightest doubt as to the phenomena; and I think there can be no question that the foregoing explanation is correct.

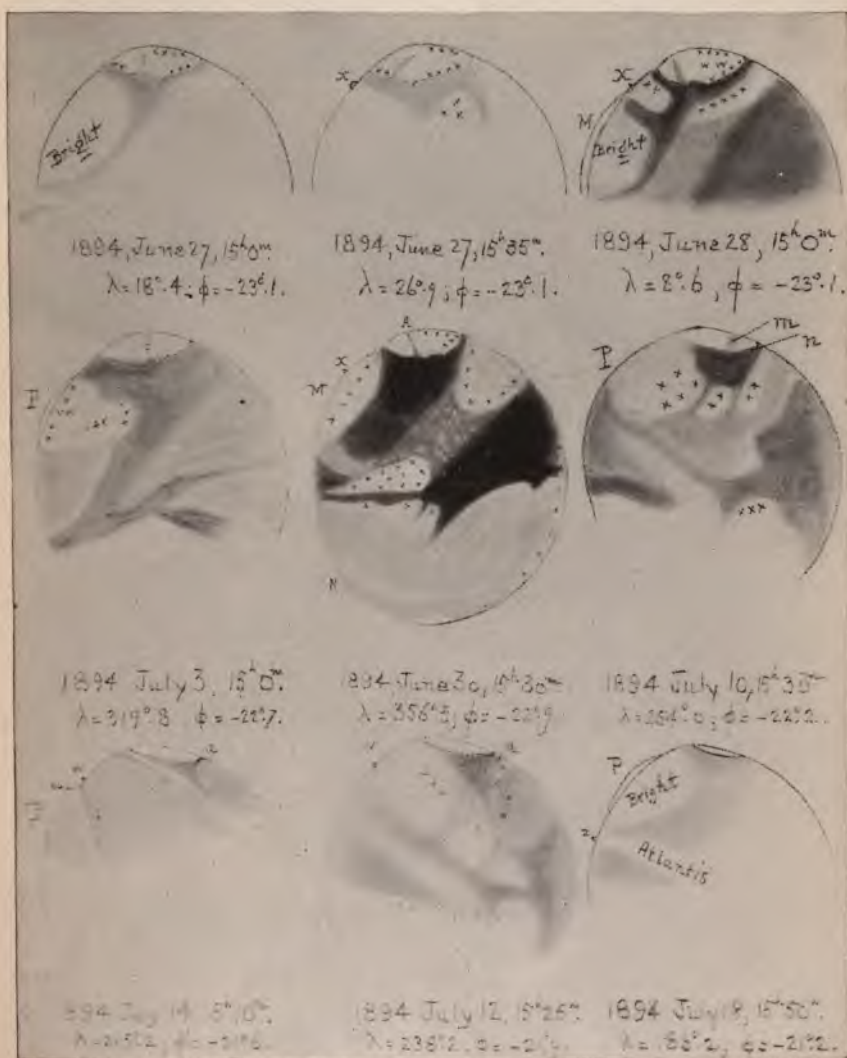
The writer of the article in *Nature* was clearly not familiar with the literature of the subject. His remarks are given under the striking title "A Strange Light on Mars." After reciting the telegram from Nice he concludes that the news must be accepted seriously and further details awaited anxiously. He concludes further that the light "must either have a physical or a human origin; so that it is to be expected that the old idea that the Martians are signaling to us will be revived." Of physical origins he suggests three, namely aurora, a long range of snow-capped hills, and forest fires. Irradiation is not mentioned. "Without favoring the signaling idea, before we know more of the observation, it may be stated that a better time for signaling could scarcely be chosen."

It will probably appear unnecessary to anyone who has read the article by Professor CAMPBELL, previously cited, to go so far afield for explanations. To anyone who has actually observed the phenomena in question, night after night, as has been done by all of us at Mount Hamilton, the suggestion that these prominences are signal lights from (possible) inhabitants of *Mars* is simply preposterous. I can find no milder term.

There are at least five or six telescopes in England which will show these prominences, and as many more on the Continent. So far as I know they have been seen in 1894 only at Mount Hamilton, Nice, and at the Lowell Observatory in Arizona.

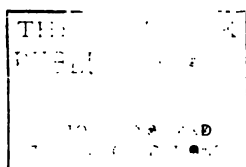
E. S. H.

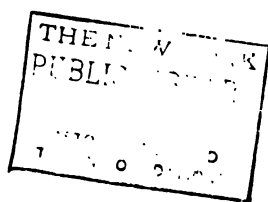
The LICK OBSERVATORY, August 24, 1894.



DIAGRAMS OF MARS, SHOWING BRIGHT PROMINENCES.

By EDWARD S. HOLDEN.







A. COCL S. J.

FROST'S TRANSLATION OF SCHEINER'S SPECTRAL-ANALYSIS.

Professor E. B. FROST, of Dartmouth College, has done a great service to English speaking astronomers and spectroscopists by translating Dr. SCHEINER's *Die Spectralanalyse der Gestirne*. The original work appeared in 1890 and at once took an important place as a text-book for students and as a summary of results for astronomers. The science of spectroscopy is advancing so rapidly in precision and so many new results have been acquired that a new edition was much needed. Mr. FROST's translation is, in fact, a complete revision; and the work in its present form brings the subject well up to date, and presents it in a very convenient form. At a future time, it is hoped to print in these *Publications* an adequate review of this treatise. For the present, we must congratulate the author, the translator, and the publishers on its appearance. It should be in all astronomical libraries.

It is an interesting evidence of the increased attention paid to astronomy in the United States that this treatise was first translated by an American and issued by an American publisher.

E. S. H.

August 15, 1894.

MAP OF THE NEIGHBORHOOD OF MOUNT HAMILTON.

Mrs. CAROLINE STEVENS WALTER, the editor of the *Santa Clara*, has kindly presented to the A. S. P. the electrotype from which the accompanying map of the vicinity of Mount Hamilton is printed. Many of our foreign members will be glad to see this diagram which shows how the Observatory is situated with respect to neighboring towns.

E. S. H.

VISITORS TO THE LICK OBSERVATORY.

The Society of "Native Sons" of California met in San José on September 9, 10, 11. At the request of the local Committee on Arrangements, they were invited to the Observatory on September 11, and a party of fifty took advantage of the invitation.

E. S. H.

APPARATUS FOR PHOTOGRAPHING METEORS.

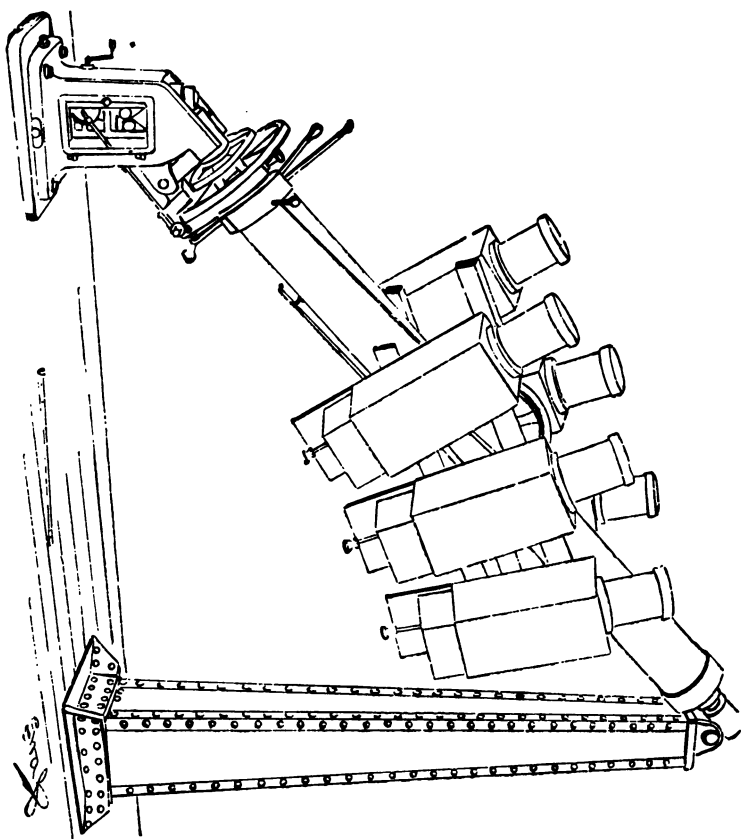
"A new instrument, termed a sextuple photographic telescope, is about to leave this city for New Haven, Conn. It will be

placed in Yale College Observatory, and though intended for general astronomical photography, will begin its picture-taking with the appearance of the August meteoric displays. The instrument was made by WARNER & SWASEY, and was boxed for shipment yesterday. In certain respects it stands unique among photographic telescopes. It has six cameras, instead of one, and together they cover a field in the sky equal to that which would be occupied by 2400 full moons. By its use on the meteors the astronomers of Yale expect to develop some new facts relating to their heights. They will place single telescopes at distances of two miles from the sextuple instrument, and, by means of electrical connections, will photograph simultaneously. The plates will then be compared, and calculations will be made with a view to establishing the height of meteors.

In the new astronomy, in which is included astronomical photography, the sensitive plate often has to be exposed from one to three hours to enable it to gather the light from the finest stars and imprint it on the plate. This is necessarily a tedious piece of work for the astronomer, as he must watch the plate very carefully during the entire exposure. Dr. W. L. ELKIN, of Yale College Observatory, wishing to accomplish more in the same space of time, devised the plan by which six photographic telescopes could be placed in one mounting. Each of these carries a plate which will cover a space of ten degrees square in the heavens. These cameras are so arranged as to have the field of one just reach the edge of the field of its neighbor, so that at one setting the six photographic telescopes will cover a field of 600 square degrees in the sky, and, as they are all carried on one equatorial mounting, the care exercised in operating them is no more than if a single telescope was being used. This plan of Dr. ELKIN has been carried out at the works of WARNER & SWASEY.

The picture illustrating this unique instrument will be easily understood by any one familiar with astronomical instruments. The long axis supported at both ends is set so as to point exactly to the north pole of the heavens. This is made to revolve by means of a driving clock inside the small column at the left hand of the instrument. The weight which serves as a motive power for this driving clock is carried in the tall column at the right hand side of the illustration. The instrument is provided with finely graduated circles so as to enable the astronomer to set

THE SEXTUPLE PHOTOGRAPHIC TELESCOPE.



the photographic telescopes on any portion of the heavens he desires.

A dull black finish was given to the tubes of the telescope, and this, contrasting with the brightly polished brass of the clock work and wheels, combines to make the instrument a handsome one. When in place it will be furnished with lenses and plates suited for the work to which it is intended. One operator will be sufficient to manage it, inasmuch as the plates can be placed in readiness before the exposure, and the handling of the telescope will be rendered easy. Professor NEWTON, who is also deeply interested in meteors, will co-operate with Dr. ELKIN. They expect to attain valuable results."—From the *Cleveland Leader*, July 18, 1894.

EXTRACT FROM A LETTER FROM M. FLAMMARION RELATIVE
TO THE ECLIPSE COMET OF 1893. AND TO BRIGHT
PROJECTIONS ON THE TERMINATOR OF *MARS*.

[*Translation.*]

"OBSERVATORY OF JUVISY, August 28, 1894.

"DEAR MR. HOLDEN:

"As soon as your letter, relating to the small comet detected on the American negatives of the eclipse, was received, I asked for the information which you desired regarding the negatives of the two French expeditions to Senegal. All the plates have been examined with care, but nothing has been found on any of them. Possibly the comet had passed behind or in front of the Sun at the hour of the Senegal observations. Perhaps, again, the meteorological conditions at Senegal were the cause. You know that the atmosphere was [then] very highly illuminated.

"We are now making some interesting observations of *Mars*. During the night of 22 to 23 August, we—M. ANTONIADI and myself—have seen a swelling of the terminator towards 45° Lat. S., and 355° Long., which corresponds to *Noachis*. It seems that there are surely mountains in this place.

* * * * *

C. FLAMMARION."

PARTIAL ECLIPSE OF THE MOON, SEPTEMBER 14, 1894.

The *American Ephemeris* gives the following Greenwich Mean times of the phases of the partial eclipse on September 14 :

	H.	M.
Moon enters penumbra,	13	58.6
Moon enters shadow,	15	35.6
Middle of the eclipse,	16	31.6
Moon leaves shadow,	17	27.7
Moon leaves penumbra,	19	4.4

The Pacific Standard times are 8^h slower than the above.

C. D. P.

PLATE OF THE MOON.

The plate of the Moon printed in the present number is from an electrotype kindly presented to the LICK Observatory by Mr. S. S. McCLURE of New York City. It is copied from one of the negatives of the LICK Observatory and has been previously printed in *McClure's Magazine*.

E. S. H.

PORTRAIT OF MR. LICK.

The bronze portrait medallion of Mr. LICK at the Observatory, was copied by the half-tone process and printed in No. 33 of these *Publications*, and also on the title-page of Volume III of the *Publications* of the LICK Observatory. As it was not entirely satisfactory, a wood-cut has been made from a negative of the original medallion (taken by Mr. COLTON) by the kindness of the editors of the *Century* magazine, and this is printed in the present number. The wood-cut is a gift from Hon. W. W. LAW to the LICK Observatory.

E. S. H.

ILLUSTRATIONS OF THE LICK OBSERVATORY STATION
IN CHILE.

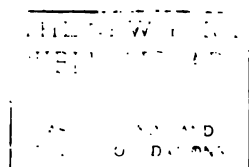
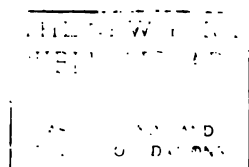
The three illustrations (*l*), (*m*), and (*n*), reproduced from photographs taken at the LICK Observatory station in Chile, and primarily intended for the "Report on the Eclipse of April 16, 1893," to be issued by the LICK Observatory, are here published in advance for the benefit of the members of the Society.

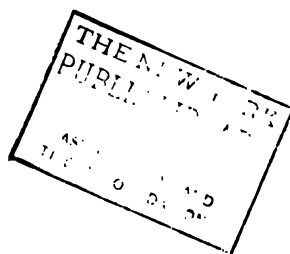
Plate (*l*) represents the first general view of the mining camp which the traveler obtains in following the trail from the railroad terminus at Merciditas to Mina Bronces.



LICK OBSERVATORY ECLIPSE STATION, APRIL 16, 1906.

(Mina Bronces, Chile.)







MINA BRONCES, ATACAMA, CHILE, APRIL 16, 1893.
The cross is above the Lick Observatory Eclipse Station.

The camp proper lies in and near the head of a cañon on the west face of Cerro Cobre mountain at an altitude of 6600 feet. The location of the LICK Observatory station is indicated by the cross + and also by the outline of a portion of the 40-foot telescope.

The barren and desolate aspect of the landscape is everywhere the same for hundreds of miles along the coast. Trees and other useful vegetation are only to be found in the narrow beds of the comparatively few watered valleys whose sources are in the Cordilleras.

Plate (*m*) gives a general view of the station.

The arrangement of the eye-end interior of the 40-foot telescope is shown in Plate (*n*).

Near the lower extremity of the fixed wooden structure, enclosing and supporting the canvas tube, the rather curiously-shaped framework (for holding the angle-iron guides for the triangular slide carrying the sensitive plates), resting on three legs, was securely fastened to the ground with a liberal supply of mortar.

The frame was, however, first approximately oriented so that the plane of the slide was normal to the axis of the telescope, and the direction of the guides parallel to motion the Sun's image would have on the day of the eclipse. The final adjustments were made by shifting the iron slides on the frame with suitable adjusting screws. Three carefully made wheels, whose axes were fastened to the triangular slide, served to carry the latter.

The two lower wheels had knife-edges working in a V groove cut in the upper face of the lower guide, the third wheel simply rolled on the planed face of the upper guide. Directly on top of the highest leg, the clock portion of one of our chronographs was mounted. The unwinding of a strong flexible wire wound around a drum on the clock's winding axis served to regulate the velocity of descent of the slide to which one extremity of the wire was fastened. The action of gravity on the slide alone was the motive power for running the regulating clock.

The line joining the supports for the photographic plates was made parallel to the line of motion, so that the longer edges of the plates should be parallel to the equator during the exposures.

The objective remained uncovered during the whole time of totality. Each of the eight plates exposed was in its own paste-board box, the cover of which could be quickly removed and

replaced for the exposures. These boxes were placed in a pile in one corner of the room; in an adjacent corner was a wooden box into which each paste-board box was placed immediately after the exposure of its plate.

A large rock on the further lower corner of the room served as a stepping-stone to enable me to obtain a naked-eye view (during the 32^s exposure), through a peep-hole in a box-like addition on the further upper corner of the room. During totality I was alone in the telescope and there was not the slightest hitch in the programme. The necessary signals at the beginning and end of totality were given to the observers at the other instruments at the proper instant by visual observation of the large focal image.

J. M. S.

MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, IN SAN FRANCISCO, IN AUGUST, 1895.

It is practically decided that the A. A. A. S., under the presidency of Professor E. W. MORLEY, Adelbert College, Cleveland, is to meet in San Francisco in August, 1895. The list of officers of the meeting is printed below. The place of the meeting is very far removed from the residence of most of the members, and the attendance is not likely to be numerically as large as at a meeting in Brooklyn or in Washington. It is therefore all the more important that the quality of the papers presented should be high, and their number considerable. The universities and colleges of California and the neighboring States and the various scientific societies must look to it that a dignified and adequate representation is made of the science of the Pacific Coast.

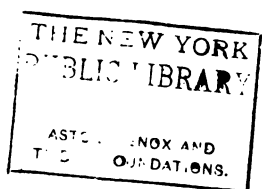
The Section of Mathematics and Astronomy will probably hold at least one meeting at Mount Hamilton, and it will probably be desired to hold a meeting of the Astronomical Society of the Pacific at the same time and place. At the proper time the undersigned will ask the co-operation of his fellow-members of the Society in specific ways. The general announcement of the meeting of the A. A. A. S. is all that needs to be made at this time.

E. S. H.

LICK OBSERVATORY, October 1, 1894.



ECLIPSE OBSERVATORY ECLIPSE STATION APRIL 26, 1959



OFFICERS ELECTED FOR THE MEETING OF THE A. A. A. S.
IN 1895.

President—E. W. MORLEY, Cleveland, Ohio.

Vice-Presidents—(A) Mathematics and Astronomy—E. S. HOLDEN, Mount Hamilton, Cal. (B) Physics—W. LECONTE STEVENS, Troy, N.Y. (C) Chemistry—WILLIAM MCMURTRIE, Brooklyn, N.Y. (D) Mechanical Science and Engineering—WM. KENT, Passaic, N. J. (E) Geology and Geography—JED. HOTCHKISS, Staunton, Va. (F) Zoölogy—D. S. JORDAN, Palo Alto, Cal. (G) Botany—J. C. ARTHUR, Lafayette, Ind. (H) Anthropology—F. H. CUSHING, Washington, D. C. (I) Economic Science and Statistics—B. E. FERNOW, Washington, D. C.

Permanent Secretary—F. W. PUTNAM, Cambridge, Mass.

General Secretary—JAS. LEWIS HOWE, Louisville, Ky.

Secretary of the Council—CHARLES R. BARNES, Madison, Wis.

Secretaries of the Sections—(A) Mathematics and Astronomy—E. H. MOORE, Chicago, Ill. (B) Physics—E. MERRITT, Ithaca, N. Y. (C) Chemistry—W. P. MASON, Troy, N. Y. (D) Mechanical Science and Engineering—H. S. JACOBY, Ithaca, N. Y. (E) Geology and Geography—J. PERRIN SMITH, Palo Alto, Cal. (F) Zoölogy—S. A. FORBES, Champaign, Ill. (G) Botany—B. T. GALLOWAY, Washington, D. C. (H) Anthropology—ANITA NEWCOMB E. MCGEE, Washington, D. C. (I) Economic Science and Statistics—E. A. ROSS, Palo Alto, Cal.

Treasurer—R. S. WOODWARD, New York, N. Y.

OBSERVING LIST FOR THE MERIDIAN CIRCLE.

The *Nautical Almanac* for 1897 contains, in its list of standard stars, 120 whose positions have not been included in previous issues. Some of these are already upon the list, observed during the past year in one position of the instrument. Such of those remaining, as are not included in the list of the Berlin *Jahrbuch*, are placed upon the observing list for the coming year. With the additional stars, this list now contains 157; of which 15 are above $+70^{\circ}$ Declination, and will be observed also at lower culmination. Observing has already begun upon the full list, the original portion of which it is hoped will be completed in next September.

R. H. T.

LICK OBSERVATORY, 1894, October 1.

BRIGHT AURORA, AUGUST 19, 1894.

From 8 P.M. to about 10:30 P.M. on the above date an unusually bright aurora was seen here and was reported from many points. The arc was from 5° to 10° wide, and extended 25° or

30° each side of the north point. It was a greenish white in color and very vivid. Rose-colored streamers, extending toward the zenith, were visible at times, but were neither long nor conspicuous.

At 9^h 07^m $\pm$ 15^s Pacific Standard time, a bright meteor was seen to fall almost vertically from *Polaris* to the horizon, and apparently was visible until it passed behind the high range of mountains to the north; but this was not quite certain, owing to the unusual amount of smoke near the horizon.

A second meteor, not quite so bright as the first, appeared about ten seconds later and fell to the west of the first, its path inclining towards the west at an angle of 10° or 15° with the vertical.

C. D. P.

LICK OBSERVATORY, August 20, 1894.

OBSERVATIONS OF THE AUGUST METEORS OF 1894, BY MESSRS. SCHAEBERLE, COLTON, PERRINE AND POOLE.

At the suggestion of Professor SCHAEBERLE the August meteors of 1894 were observed by him from the summit of Monte Diablo, and by Messrs. COLTON, PERRINE and POOLE from the summit of Mount Hamilton, on three nights. The primary object sought for was to secure observations of the same meteor at both stations, and thus to determine the height of such bodies in the Earth's atmosphere. A considerable number of corresponding observations of the same meteor were obtained. The maps now printed give a graphical representation of the Mount Hamilton observations of Messrs. COLTON and PERRINE, (observers), and of Mr. POOLE (recorder). The maps of Professor SCHAEBERLE will be printed later, in an octavo pamphlet to be published by the Observatory. Several of the August meteors were photographed by Professor BARNARD. His results have been sent to *Astronomy and Astro-Physics*.

E. S. H.

LICK OBSERVATORY, October 4, 1894.

ERRATA IN *PUBLICATIONS* No. 37.

Page 238, last line, *for* August *read* October. E. S. H.

In the set of star charts printed in No. 37 of these *Publications*, there is an extra star shown in Chart VIII, following δ *Ophiuchi*.

It is at the top of the map, 3.2 inches (1.6 minutes of A. R.),

from the right (preceding) end; and is represented as of the lowest grade, 16-17 magnitude.

There is no trace of this star upon the original charts, from which this print was copied, and it is probably due to a speck of dust upon the photographic plate.

R. H. T.

THE ECLIPSE-COMET OF APRIL 16, 1893.

A note from Professor KRUEGER, editor of the *Astronomische Nachrichten*, dated 1894, August 11, relative to the nomenclature of the above mentioned comet, reads as follows (in translation):

"It will not do to designate the comet as 1893, I, as this would then necessitate a change in the other comets, which would result in great confusion. Again, this is contrary to the custom heretofore in vogue, as no orbit computations are available, and the time of perihelion cannot be given. It will have to be treated like the eclipse-comet of 1882, SOHAG, which likewise could not be designated by a number."

In the *Astronomische Nachrichten* No. 3253, just received, Professor KRUEGER refers to copies of the LICK Observatory and British plates made by Professor SCHAEBERLE and sent to him, and says that on these copies the comet "can plainly be seen," and again, that the British plates show it "undoubtedly." It is difficult to understand how this object was overlooked in the examinations made by the English observers.

E. S. H.

BRIGHT METEOR OF JULY 29, 1894.

I first saw this meteor in *Cepheus*, at 9^h 30^m 08^s Pacific Standard time, from whence it passed through the northern end of *Cassiopeia* and burst, in *Camelopardalis*, into several fragments. I heard no noise. The color was bluish-white. It left a red train which was visible for two or three seconds. Its path was sinuous. It was a very conspicuous object, being many times as bright as *Venus*, sufficiently bright to cast quite a perceptible shadow.

C. D. P.

LICK OBSERVATORY.

"OAKLAND, Cal., July 31, 1893.

"Dear Sir: Sunday, July 29, about 9:40 P.M., I saw a meteor which I think was equal to that of Friday, July 27, and probably belongs to the same swarm. I was, at the time, at a point about half a mile north of Haywards, when the surrounding

country and sky became suddenly illuminated. Looking east, I saw, about 30° above the horizon, a meteor consisting of a white incandescent globe followed by a cone of dazzling blue fire. It was moving nearly north, and apparently burned itself out while passing through or near the constellation *Cassiopeia*.

"Yours respectfully, E. A. RICHON.

"1112 Linden Street, Oakland, Cal."

ENLARGEMENTS OF LUNAR PHOTOGRAPHS BY PROFESSOR W. PRINZ.

Professor PRINZ of the Royal Observatory of Brussels, has presented to the Astronomical Society of the Pacific, to the Library of the LICK Observatory, and to the astronomers of the latter institution, copies of four folio plates relating to enlargements of a lunar negative made at the LICK Observatory and presented to the Royal Observatory of Brussels, for which our thanks are returned. This atlas was received at Mount Hamilton after Vol. III of the *Publications* of the LICK Observatory had been issued—too late, therefore, to be referred to in the introduction to that volume. The atlas consists of four sheets. The first is a reproduction (not enlarged) of the original negative; and contains a preface by Professor PRINZ which describes the methods of enlargement and reproduction employed, etc.

Plate I is an 8-fold enlargement of *Mare Imbrium* and vicinity; II is a 24-fold enlargement of *Bullialdus*, etc.; III is a 33-fold enlargement of *Copernicus*.

These plates are interesting as showing the best results which can be obtained from the LICK Observatory negatives by the method employed (*phototypie*). The heliogravure process employed in making the plates of Vol. III of the *Publications* of the LICK Observatory is certainly to be preferred.

EDWARD S. HOLDEN.

LICK OBSERVATORY, September 8, 1894.

REFLECTING TELESCOPE FOR SALE.

Dr. JAMES M. BARRICKMAN (Truckee, Cal.) has a $6\frac{1}{2}$ -inch reflecting telescope for sale. Under suitable conditions he will lend it to any observer who can use it. Correspondence should be addressed to him, as above.

E. S. H.

NOTE ON AURORA OBSERVATIONS.

Those co-operating, and others, will be interested to learn that the records of observations of the aurora made at the station of Mr. PEARY in North Greenland the past winter have been received and that their comparison with similar records from other parts of the earth is now in progress. It already appears that the conclusions heretofore announced in regard to certain phases of the subject are being substantially confirmed, and that the forces of which the aurora is the visible expression and type play an exceedingly important part in many ways. The relations to certain very definite solar conditions, and to thunderstorms, and to certain phases of atmospheric control are becoming especially clear. It is fitting that all who have so kindly contributed to the success of the scheme should have assurance that their labor is not in vain. It is proposed to continue these observations, and if possible make them even more valuable. All data bearing upon the subject will be thankfully received and employed to the best possible advantage. M. A. VEEDER.

LYONS, N. Y., U. S. A., Oct. 18th, 1894.

TRANSIT OF *MERCURY*, OBSERVED AT THE LICK OBSERVATORY, NOVEMBER 9-10, 1894.

Observations of the Contacts. By Professor SCHAEBERLE.

I.	19 ^h 57 ^m 15 ^s	P. S. T.
II.	19 58 26	Geometrical contact.
II.	19 58 52	
III.	1 11 35	
IV.	1 13 21	

The above observations were made with the photoheliograph, aperture 3 inches.

Contacts I and II were observed on the focusing screen with the naked eye. For contacts III and IV the focal image on the screen was viewed with a hand lens of about 3 inches focus.

Observations of the Contacts. By Professor BARNARD.

I.	19 ^h 57 ^m 9 ^s .4	P. S. T.
II.	19 58 42.7	Geometrical.
III.	1 11 37.1	Geometrical.
IV.	1 13 16.7	

Fifty-three measures of the Right Ascension and Declination diameters of the planet were made; and also forty-eight measures of the position of *Mercury* on the Sun's disc.

The above observations were made with the 12-inch equatorial.

Observations of the Contacts. By Mr. CAMPBELL.

I.	19 ^h 57 ^m 12 ^s	May be a few seconds late.
II.	19 58 23	Geometrical.
II.	19 58 39	First light between limbs.
III.	1 11 36.4	Last light between limbs.
III.	1 12 3.4	Geometrical.
IV.	1 13 11.4	

Eighteen measures of *Mercury's* position with reference to the Sun's limb were made. Sixty-two measures of the diameter of the planet gave 5".73 at distance unity.

The observations were made with the great equatorial, aperture 8 inches, power 350 at ingress and 270 at egress.

Observations of the Contacts. By Mr. TUCKER.

I.	19 ^h 57 ^m 11 ^s .8	P. S. T. Distinctly on limb.
II.	19 58 41.8	
III.	1 11 33.3	
IV.	1 13 7.3	

The time recorded for II and III is that at which the two limbs exactly touched, without any light being visible between. No sensible distortion of the planet was noted.

The above observations were made with the 6-inch finder of the great telescope, with the aperture reduced to 3½ inches, power 90. The Sun and *Mercury* were also observed in transit with the meridian circle, and *Mercury* was observed for Declination.

Observations of the Contacts. By Mr. PERRINE.

I.	19 ^h 57 ^m 16 ^s .1	P. S. T.
II.	19 58 30.1	Geometrical contact.
II.	19 58 57.1	
III.	1 11 25.1	
III.	1 11 42.1	Geometrical contact.
IV.	1 13 15.1	

The above observations were obtained by viewing the focal image of the photoheliograph on the focusing screen, in the case



LICK OBSERVATORY, OCTOBER 11, 1891.

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PUBLIC LIBRARY

ASTOR LENOX AND
TILDEN FOUNDATIONS.

of contacts I and II, with the naked eye, the effective magnifying power of the telescope being about 50; contacts III and IV were observed in the same way except that a hand magnifier of a little more than 3 inches focus was used, giving a power of about 140. The aperture of the photoheliograph was 3 inches.

Corrections to the Lick Observatory Railway Time-Signal.
By Mr. TUCKER.

November 9, at noon, correction to P. S. T. = $-0^s.03$.

November 10, at noon, correction to P. S. T. = $+0^s.11$.

As this clock was used in part of the observations the rate was not changed during the interval.

Photographs of the Transit with the Photoheliograph. By
Professor SCHAEBERLE and Mr. PERRINE.

Thirty-eight negatives of the transit were taken with the photoheliograph. Not all of them are developed at the time of writing. Those that have been examined are all suitable for measurement.

PICTURE OF THE GREAT METEOR OF JULY 27, 1894.

The excellent cut of the meteor of July 27, 1894, is copied from a water-color sketch made by Mr. CHAUNCEY M. ST. JOHN, which has been presented to the LICK Observatory by the artist. It is an exceedingly faithful and artistic representation of the meteor and will serve to recall the phenomenon to all who saw it from the neighborhood of Oakland, etc. It is here printed facing page 268.

E. S. H

CORRECTIONS TO HUSSEY'S LOGARITHMIC TABLES.

In the second edition, $\log. \sin 37^\circ 4'$ should be 9.78013. The characteristic of $\log. \sin 79^\circ$ should be 9. These are the only numerical errors that have been reported in the first 145 pages of the book. In a few cases type of wrong font have been used; this, however, will not lead to errors in computation. On page 146, line 25, $497''.78$ should be $497^s.78$; and, on page 148, the velocity of light should be 29.9853 km. per second. The value of the centimetre in inches is given, on page 146, as .39370432. This is CLARKE's value. It has been shown that the last three decimal places of this value are very uncertain.

The Bureau of Weights and Measures of the United States Coast and Geodetic Survey has provisionally adopted the value .393700. If this value were assumed instead of CLARKE'S some of the metric equivalents on pages 146-8 would have slightly different values.

W. J. HUSSEY.

STANFORD UNIVERSITY, 1894, November 19.

THE CHEMICAL CONSTITUTION OF *MARS*' ATMOSPHERE,
BY DR. HENRY H. BATES.

[The following private letter from one of the members of the Society is interesting; and especially so if read in connection with the article by Professor CAMPBELL (*Concerning an Atmosphere on Mars*), printed in this number of the *Publications*. It is proper to say that the conclusions which Dr. BATES has independently reached have been suggested and discussed by others. Professor SCHIAPARELLI, Mr. A. COWPER RANYARD and others have printed papers or paragraphs on the subject; and the general question of a substitute of other chemical compounds for water has been considered for some time past in the LICK Observatory. E. S. H.]

"PROFESSOR E. S. HOLDEN, Director LICK Observatory.

"I was very much impressed with the * * * paper by Mr. CAMPBELL on the spectrum of *Mars*, reported in No. 37 of the *Publications* A. S. P. This * * * seems to entirely negative former impressions as to evidence of aqueous vapor on *Mars*. Mr. CAMPBELL himself seems to have worked under a foregone conclusion opposed to his proofs, which makes his work all the more trustworthy. It is a remark at the close of his paper that I wish to question. He considers, with many others, that 'the polar caps on *Mars* are conclusive evidence of an atmosphere *and aqueous vapor*.' Is not this putting it very strongly in the absence of all positive evidence and in the face of the powerful adverse negative evidence? The only reason for believing in the presence of aqueous vapor on *Mars*, is the waxing and waning of the white polar caps under the Sun's apparent influence, in analogy with those of our planet.

"But why assume snow or ice? It seems incredible from *Mars*' small size and great distance from the Sun, and relatively scant atmosphere, that it can have sufficient contained heat, or can receive sufficient heat from any source to liquefy ice, even if it ever had an aqueous atmosphere. The amount of solar heat received is calculable and is insufficient. The temperature of *Mars* at the

poles cannot be much above absolute zero point. It is chilled to the center and is cracked through and through in many directions, as the immense cañons, absurdly called canals, evince. So much so, that it would probably go to pieces on any unusual gravitative-deforming stress, like its outer neighbor whose wreck formed the debris of the asteroids. There is no reason to suppose that white caps necessarily imply ice. I consider that instead of oxide of hydrogen these caps are more likely composed of carbon dioxide, or at least some oxide or salt whose solid state and liquid and gaseous states are more in accord with the actual temperature conditions of *Mars*. Caps of snow-like carbonic acid would wax and wane under the very small accession of heat possible on *Mars*, precisely as do snow caps under our much higher temperature conditions. From this might result the floods of liquid carbon dioxide, which appear to fill the cañons and low places in variable amount, and might also be the source of a low and dense atmosphere of carbonic acid vapor, sufficient to explain the atmospheric indications perceived on *Mars*, especially the refractive duplication of the cañon lines.

"Another consideration negatives the antecedent probability of the existence of watery vapor on *Mars*. The mass of *Mars* is quite too small to have retained a gaseous envelope constituted like our atmosphere—certainly so far as regards free hydrogen, and probably also free oxygen, as shown by the late J. G. STONEY. The constituents of water therefore being *ab origine* absent, the presence of water cannot be predicated, unless we imagine oceans of aqueous substance (hail) wandering in space ready-made to be precipitated upon convenient planets, which is too violent a supposition.

"*Mars* is no doubt in a more senile and decrepit condition than our Moon. The Moon indeed is entirely dead, but it is not *cracked* and ready for disintegration apparently. It long had a considerable thermal retarding influence in its near companion, the Earth. *Mars*, though larger, is far worse off, from its long isolation, though from this very fact *Mars*, unlike the Moon, could have retained some atmosphere of heavy, dead and dense gases enough to account for the observed phenomenon of white precipitation at the points of absolute cold. It is for observers to identify this substance chemically. I should wish very much that * * * Mr. CAMPBELL would try for spectroscopic indications of carbon lines, or lines of any other haloid that solidifies

only at extremely low temperatures and becomes liquid and gaseous at such temperatures as might reasonably be looked for on *Mars*.

I am very sincerely yours,

HENRY H. BATES."

WASHINGTON, D. C., September 8, 1894.

ORBIT OF COMET ϵ , 1894 (E. D. SWIFT, NOVEMBER 20). BY
PROFESSOR A. O. LEUSCHNER.

From three observations by Professor BARNARD at Mount Hamilton, I have computed the following parabolic elements :

Perihelion passage, 1894, Oct. 4.9009, Berlin Mean Time.

Longitude Node, $43^{\circ} 4' 17''$

Longitude Perihelion, $334^{\circ} 52' 44''$

Inclination, $3^{\circ} 16' 13''$

Perihelion distance, 1.4703.

DEPARTMENT OF ASTRONOMY,

UNIVERSITY OF CALIFORNIA, BERKELEY, Nov. 28, 1894.

DISCOVERY OF COMET ϵ , 1894 (SWIFT).

Mr. E. D. SWIFT telegraphs from the LOWE Observatory, Echo Mountain, California, that he discovered an unexpected comet on November 20, at $8^h 30^m$ ($16^h 30^m$, G. M. T.)

E. S. H.

BRIGHT PROMINENCES ON THE TERMINATOR OF *MARS*,
SEEN AFTER OPPOSITION.

The first bright prominence on the terminator of *Mars* to be seen after opposition, appeared November 20, and was seen by Professor SCHAEBERLE.

E. S. H.

THE ECLIPSE COMET OF APRIL, 1893.

The *Observatory* for November, 1894, contains an important note by Mr. W. H. WESLEY, Assistant Secretary of the Royal Astronomical Society, in which he gives measures (and diagrams) of the eclipse comet as shown on the negatives of the British expeditions to Brazil and Africa. These independent measures by Mr. WESLEY on the originals agree substantially with the measures made by Professor SCHAEBERLE on the copies furnished to the LICK Observatory.

E. S. H.

LICK OBSERVATORY, November 16, 1894.

DECORATION FOR PROFESSOR HOLDEN.

The Secretary of State of Saxe-Meinigen has transmitted to the Director of the LICK Observatory the diploma and cross of Commander of the Saxe-Ernestine Order, II Class. This order was founded in 1690 and reorganized in 1833, and is given in recognition of distinguished service in high official positions, either military or civil.

ERRATUM.

In these *Publications*, Vol. VI, No. 37, page 194, line 20, for "crosses the equator" read "reaches the solstice." M. M.

MINUTES OF THE MEETING OF THE BOARD OF DIRECTORS,
HELD AT THE LICK OBSERVATORY, SEPTEMBER 1, 1894.

President CAMPBELL took the chair; and a quorum was present. The minutes of the last meeting were approved. The following members were elected:

LIST OF MEMBERS ELECTED SEPTEMBER 1, 1894.

Dr. V. CERULLI	Teramo, Italy.
Mr. P. HENRY GRIFFIN	{ Combe Wood, Bonchurch, Isle of Wight, England.
Mr. WM. RUMFORD HOWELL	{ 4306 Walnut Street, Philadel- phia, Pa.
Mr. JAMES R. KELLY	{ 309 Leavenworth Street, San Francisco, Cal.
Mr. ALBERTO LOLOIR	{ 625 Calle Piedad, Buenos Ayres, Republica Argentina.
Mr. WILLIAM LEVISTON	{ 508 California Street, San Fran- cisco, Cal.
Mr. PATRICK MACMANUS	{ 1294 Calle Piedad, Buenos Ayres, Republica Argentina.
Dr. JORGE C. MONZON	{ Quezaltenango, Guatemala, Central America.
Mr. ARTHUR FRENCH POOLE	{ Stanford University, Palo Alto, California.
Mr. WALTER A. POST	Newport News, Va.
Mr. FREDERICK E. REED	{ 120 Gold Street, Worcester, Mass.
Mr. CHAUNCEY M. ST. JOHN	{ U. S. Custom House, San Fran- cisco, Cal.

It was decided to postpone, until the next meeting, action on the resolution providing for the transference each year of a portion of the General Fund to the Life Membership Fund.

It was *Resolved*, That the *Publications* of the Astronomical Society of the Pacific be entered at the Postoffice in San Francisco as second-class matter, as provided for by Act of Congress, approved July 16, 1894, and that to comply with the postal regulations the dates of publication shall be as follows: February 1, April 1, June 1, August 1, October 1, and December 1.

Adjourned.

MINUTES OF THE MEETING OF THE ASTRONOMICAL SOCIETY
OF THE PACIFIC, HELD AT THE LICK OBSERVATORY,
SEPTEMBER 1, 1894.

Professor CAMPBELL presided. The minutes of the last meeting as printed in the *Publications*, were approved.

The Secretary read the names of members duly elected at the meeting of the Directors.

The following papers were presented:

1. Planetary Phenomena for November and December, 1894, by Professor MALCOLM MCNEILL, of Lake Forest, Ill.
2. Photographic Observations of *Algol*, by Mr. S. D. TOWNLEY, of Ann Arbor, Michigan.
3. Remarks on the preceding paper, by Professor J. M. SCHAEBERLE, of the LICK Observatory.
4. Stellar Parallax, by Professor F. P. LEAVENWORTH, of the University of Minnesota.
5. Can Organic Life exist in the Solar System anywhere but on the Planet *Mars*? by M. Camille Flammarion, of Paris.
6. Lunar Eclipses and some similar Phenomena, by Mr. W. H. S. MONCK, of Dublin.
7. Cloud Observations on the Island of Yezo, Japan, with reference to the Solar Eclipse of 1896, August 9, by Professor W. W. CAMPBELL, of the LICK Observatory.
8. Predictions for the Transit of Mercury, 1894, November 10, by Mr. O. E. HARMON, of Chehalis, Washington.
9. The Arequipa Observations of *Mars* and *Jupiter*, by Professor W. H. PICKERING, of Harvard University.
10. Addendum to the above, by Professor E. S. HOLDEN, of the LICK Observatory.
11. Charts of Faint Stars for Magnitude Comparison (Second Series), by Mr. R. H. TUCKER, Jr., of the LICK Observatory.
12. Investigation of the Spectrum of the Planet *Mars*, by Professor W. W. CAMPBELL, of the LICK Observatory.

Adjourned.

MINUTES OF THE MEETING OF THE BOARD OF DIRECTORS,
HELD IN THE ROOMS OF THE SOCIETY, NOVEMBER
24, 1894, AT 7:30 P. M.

President CAMPBELL presided. A quorum was present. The minutes of the last meeting were read and approved. The following members were duly elected:

LIST OF MEMBERS ELECTED NOVEMBER 24, 1894.

Dr. JAMES M. BARRICKMAN	{ General Delivery, Post Office, San Francisco, Cal.
Il Conte CARLO CAISSOTTI DI CHIUSANO, Via Bogino, 12; Torino, Italia.	
Mr. JOHN EVERDING JR.	48 Clay Street, San Francisco, Cal.
Mr. J. O'B. GUNN	Box 2128, San Francisco, Cal.
Mr. WM. J. LANDERS	{ 205 Sansome Street, San Francisco, Cal.
Miss KATHERINE M. LANE	{ 144 West State St., Trenton, New Jersey.
Mr. LOUIS C. MASTEN	{ 2218 Clay Street, San Francisco, Cal.

The election of Messrs. BARRICKMAN, CAISSOTTI, EVERDING and MASTEN and of Miss LANE is to date from January 1, 1895.

The following resolution was unanimously adopted by the consenting votes of the Directors, namely, Messrs. PIERSON, DOLBEER, VON GELDERN, ALVORD, MOLERA, ZIEL, HUSSEY, HOLDEN, PERRINE, CAMPBELL, SCHAEBERLE :

Resolved, That the Treasurer be and he hereby is authorized to withdraw from the Life Membership Fund (on deposit with the San Francisco Savings Union) such sums of money, not to exceed a total of Three Hundred Dollars, as will be necessary to pay the expenses of the General Fund, during the current fiscal year.

Adjourned.

MINUTES OF THE MEETING OF THE ASTRONOMICAL SOCIETY OF THE PACIFIC, HELD IN THE ROOMS OF THE TECHNICAL SOCIETY OF THE PACIFIC COAST, NOVEMBER 24, 1894.

The meeting was called to order by President CAMPBELL. The minutes of the last meeting were read and approved. The Secretary read the names of new members duly elected at the Directors' meeting.

The following papers were presented :

1. The LICK Observatory Eclipse Expeditions of January, 1889, December, 1889, and April, 1893, by Professor HOLDEN, Mount Hamilton.
2. Lunar Eclipses and Some Similar Phenomena, by Mr. W. H. S. MONCK, of Dublin.
3. Predictions for the Transit of *Mercury*, November 10, 1894, by Mr. ROGER SPRAGUE, of Denver.
4. Planetary Phenomena for January, February and March, 1895, by Professor M. MCNEILL, of Lake Forest.
5. The Brilliant Meteor of July 27, 1894, by Professor HOLDEN, of Mount Hamilton.
6. Observations of the Transit of *Mercury*, November 10, by Messrs. SCHAEBERLE, BARNARD, CAMPBELL, TUCKER and PERRINE, at the LICK Observatory.
7. Observations of the Transit of *Mercury*, November 10, by Mr. BURCKHALTER, of Oakland; by Mr. ZIEL, of Alameda; by Professor R. G. AITKEN, of College Park; and Mr. ALLEN F. GILLIHAN, of St. Helena, Cal.
8. Concerning an Atmosphere on *Mars*, by Professor CAMPBELL, Mount Hamilton.

The first, fifth and eighth papers were read. Adjourned.

OFFICERS OF THE SOCIETY.

W. W. CAMPBELL (Lick Observatory), ... *President*
 W. J. HUSSEY (Leland Stanford Jr. University, Palo Alto, Cal.), }
 WM. M. PIERSON (Mills Building, S. F.), } *Vice-Presidents*
 JOHN DOLBERG (10 California Street, S. F.), }
 C. D. PERRINE (Lick Observatory), ... *Secretary*
 F. R. ZIEGL (410 California Street, S. F.), ... *Secretary and Treasurer*

Board of Directors—Messrs. ALVORD, CAMPBELL, DOLBERG, HOLDEN, HUSSEY, MOLERA, PERRINE, PIERSON, SCHAEFERLE, VON GELDERN, ZIEGL.

Finance Committee—Messrs. PIERSON, DOLBERG, VON GELDERN.

Committee on Publication—Messrs. HOLDEN, CAMPBELL, YALE.

Library Committee—Messrs. VON GELDERN, MOLERA, BARCOCK.

Committee on the Comet-Medal—Messrs. HOLDEN (*ex-officio*), SCHAEFERLE, BURCKHALTER.

OFFICERS OF THE CHICAGO SECTION.

Executive Committee—RUTHVEN W. PIKE.

OFFICERS OF THE MEXICAN SECTION.

Executive Committee—CAMILO GONZALEZ, FRANCISCO RODRIGUEZ REY, AGUSTIN ARAGON.

NOTICE.

The attention of new members is called to Article VIII of the By-Laws, which provides that the annual subscription, paid on election, covers the *calendar* year only. Subsequent annual payments are due on January 1st of each succeeding calendar year. This rule is necessary in order to make our book-keeping as simple as possible. Dues sent by mail should be directed to Astronomical Society of the Pacific, 819 Market Street, San Francisco.

It is intended that each member of the Society shall receive a copy of each one of the *Publications* for the year in which he was elected to membership and for all subsequent years. If there have been (unfortunately) any omissions in this matter, it is requested that the Secretaries be at once notified, in order that the missing numbers may be supplied. Members are requested to preserve the copies of the *Publications* of the Society as sent to them. Once each year a title-page and contents of the preceding numbers will also be sent to the members, who can then bind the numbers together into a volume. Complete volumes for past years will also be supplied, to members only, so far as the stock in hand is sufficient, on the payment of two dollars to either of the Secretaries. Any non-resident member within the United States can obtain books from the Society's library by sending his library card with ten cents in stamps to the Secretary A. S. P., 819 Market Street, San Francisco, who will return the book and the card.

The Committee on Publication desires to say that the order in which papers are printed in the *Publications* is decided simply by convenience. In a general way, those papers are printed first which are earliest accepted for publication. It is not possible to send proof sheets of papers to be printed to authors whose residence is not within the United States. The responsibility for the views expressed in the papers printed rests with the writers, and is not assumed by the Society itself.

The titles of papers for reading should be communicated to either of the Secretaries as early as possible, as well as any changes in addresses. The Secretary in San Francisco will send to any member of the Society suitable stationery stamped with the seal of the Society, at cost price, as follows: a block of letter paper, 40 cents; of note paper, 25 cents; a package of envelopes, 25 cents. These prices include postage, and should be remitted by money-order or in U. S. postage stamps. The sendings are at the risk of the member.

Those members who propose to attend any or all of the meetings at Mount Hamilton during the summer should communicate with "The Secretary Astronomical Society of the Pacific" at the rooms of the Society, 819 Market Street, San Francisco, in order that arrangements may be made for transportation, lodging, etc.

PUBLICATIONS ISSUED BI-MONTHLY.



COLLEGE NOTRE DAME

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FOUNDED 1851.

INCORPORATED 1865.

STUDIES RESUMED

TUESDAY, AUGUST 7, 1894.

AMERICAN JOURNAL OF SCIENCE

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